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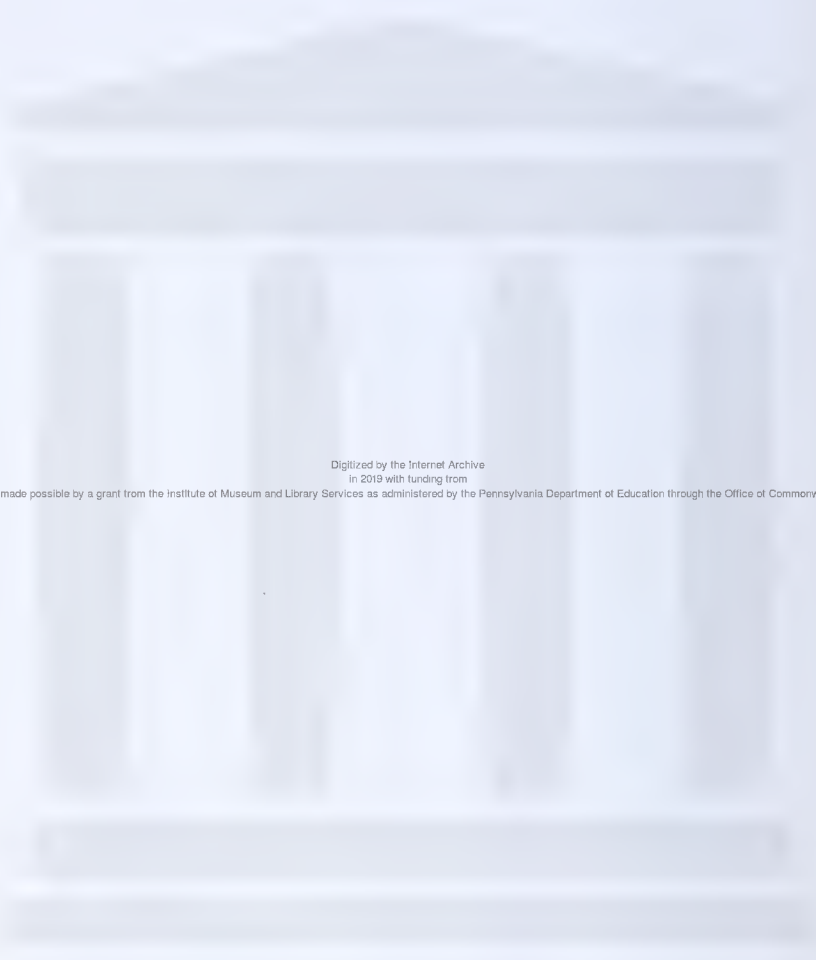
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BIOGRAPHICAL MEMOIRS

VOL. XXI

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## CONTENTS

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	Page
ERWIN FRINK SMITH.....L. R. Jones	i
EDWIN HERBERT HALL.....P. W. Bridgman	73
GRAHAM LUSK.....Eugene F. DuBois	95
ELIHU THOMSON.....Karl T. Compton	143
GEORGE ELLERY HALE.....Walter S. Adams	181
ERNEST WILLIAM BROWN.....Frank Schlesinger and Dirk Brouwer	243
JULIUS STIEGLITZ.....William Albert Noyes	275
EDMUND BEECHER WILSON.....Thomas Hunt Morgan	315
EDWARD LEAMINGTON NICHOLS.....Ernest Merritt	343

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*Ernest F. Smith*

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NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS

VOLUME XXI—FIRST MEMOIR

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BIOGRAPHICAL MEMOIR

OF

ERWIN FRINK SMITH

1854–1927

BY

L. R. JONES

*With synopsis of researches by*

ERWIN F. SMITH

*and bibliography by*

FREDERICK V. RAND

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## ERWIN FRINK SMITH

1854-1927

BY L. R. JONES<sup>1</sup>

The personal qualities that endeared Erwin F. Smith to the friends and scientific associates of his mature years were evident from his early youth. Among these were a lovable disposition, passion for study, quick idealism, intense devotion to the task in hand, and unalterable integrity. He was born in the little village of Gilberts Mills, New York, on January 21, 1854. His parents, R. K. Smith and Louisa (Frink) Smith, migrated in his early childhood to a farm home in Hubbardston in southern Michigan. Unfortunately we are able to add but little concerning his earlier ancestral history. His family were of Anglo-Saxon stock, some of the lines going back among the earliest of the New England settlers. They were pioneers and frontiersmen, who helped to settle half a dozen towns in eastern Massachusetts and then moved on into Connecticut, afterwards into central New York, and still later into southern and central Michigan and farther west. His immediate forebears on both sides lived in central New York in small farming communities, and his family had settled in Gilberts Mills shortly before he was born. Their social life centered around the church and school, and there was a strong element of piety in his home life. There was much hard work to be done, but his was a happy boyhood, with all the interests and activities connected with farming, to which he early added an interest in books, nature, science, medicine, art, and music. That Smith himself recog-

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<sup>1</sup>Thanks are due for kindly advice and aid in the preparation of this memoir to numerous friends and scientific associates of Doctor Smith. Especial mention should be made of Mrs. Erwin F. Smith for access to unpublished early writings, now deposited in the library of the U. S. Department of Agriculture, and of Dr. Liberty Hyde Bailey, whose friendship with Smith dated from their early associations as amateur botanists in Michigan. Dr. Frederick V. Rand and Miss Florence Hedges, long-time members of Doctor Smith's research group, aided concerning many details. Doctor Rand prepared the manuscript for the bibliography in consultation with Miss Claribel R. Barnett and other staff members of the Library of the Department of Agriculture.

nized his own indebtedness to worthy forebears is shown in brief suggestion at the close of his "Synopsis." There he lists two significant traits, most helpful in his scientific work, as "matters of inheritance." These were "persistence along a previously determined line of work" with a "fondness for all forms of art and a desire for perfection." Evidence for both of these traits is to be found throughout his educational development. Partly from financial necessity, partly because of shy individualism, his formal schooling was inconsequential through the Ionia (Michigan) High School from which he graduated at the unusually mature age of twenty-six. His studies were even less regular at the Michigan Agricultural College, where he spent some time while employed with the State Board of Health at Lansing. With little more formality he then enrolled at the University of Michigan and was granted the bachelor's degree in June, 1886, and three years later the doctor's degree. This latter was based on Smith's work on peach yellows, a serious orchard disease to be discussed later. At the close of the examination for the doctorate Professor Volney Spalding, his major counselor during these four years, expressed regret that the University could offer no higher token of its esteem for Smith's scholarly researches. This is but one of many testimonials that, throughout these unconventional relations in lower schools, college and university alike, he was recognized as having unusual intellectual interests and scholarly abilities. Fortunately, at all stages he also met with liberal-minded teachers and wise advisers, who gave encouragement and aid in his irregular educational programs and related problems.

From such personal associations with teachers and other friends he early developed a keen interest, continued through life, in language and literature. Similar stimulating relations on the scientific side began with Charles F. Wheeler, the druggist of his home town. Wheeler was a keen, scholarly man and the leading amateur botanist of Michigan. Through his kindly interest in this eagerly inquisitive country boy, Smith was early tutored in French and was introduced almost simultaneously to the fascinations of chemistry and botany. One result was that he early set up some simple chemical apparatus in his home.

More significant, however, was the close association of Wheeler and Smith throughout many years of intensive work in taxonomic botany. Beginning with exhaustive local explorations, these developed into a state-wide study of the Michigan flora and matured in their noteworthy hand-book, "The Flora of Michigan." This was published in 1881, the year after Smith graduated from high school. The influence of this early botanical work in association with so able and enthusiastic a taxonomist as Wheeler was evident in much of Smith's later work. It outweighed that of any formal course of study in biology.

Smith's associated school programs were at once so unusual and significant as to deserve more intimate glimpses. While his early botanical work with Wheeler was in progress he taught for some time in district schools. In 1876, when twenty-two years old, he entered the Ionia High School. Here, he found two exceptionally good teachers. The first of these was the principal, A. R. DeWolf, a recent graduate of the University of Michigan. Smith's personality and genius immediately impressed DeWolf who has, in a recent letter, described their early relations as follows:

"Wearing a full beard . . . he entered school the second week of the fall term . . . At the close of the day . . . he introduced himself . . . outlined his circumstances . . . obstacles . . . Mentioned work, upon which engaged [Flora of Michigan] . . . could not be quite regular in attendance . . . but would exercise additional diligence in preparation. His unusual intelligence, courteous bearing and evident acquirements were such that I fell in love with him and . . . gave full permission to come and go as the spirit moved . . . he dropped everything, filled his collecting can with food and went into the woods in search for uncommon and new species. For days he was lost to everything else. Before I knew him he had acquired a fine knowledge of the French language . . . [Wheeler had been his tutor and fellow correspondent with botanists in France] . . . read extensively the French scientific books, thus laying the foundation for the scientific work of his later life."

In this same Ionia school he was much influenced by an able and sympathetic English teacher, who quickly realized and stimulated his love for the best in literature, including poetry and related artistic interests. This continued as a formative influence

throughout life. Smith later commented that in these early years he would as readily have become a teacher of literature as a scientist.

Mention should here be made of another association which was to exert an important influence upon Smith's later scientific career. As earlier noted, he worked for the Michigan Board of Health at Lansing, while carrying on his undergraduate studies in the State College. This work was directed by Dr. Henry F. Baker, a recent graduate of the Michigan University Medical School, a man of scientific ability, keenly interested in the advancement of modern sanitation. To aid in such a program Dr. Baker commissioned Smith to review the literature. An important part of this was in European publications, notably German and French. Smith's exhaustive digest and forceful report upon this subject, consisting of some 180 pages, was submitted to the Board at a public meeting in 1884. The associated discussion indicates that Smith's report was accepted as a major contribution. This seems especially significant when one finds on the same program a paper by Dr. Victor C. Vaughan, then one of the keenest younger members of the University medical faculty who, with Frederick V. Novy, was soon to lead in Michigan's notable program in medical bacteriology. The influence of this experience on Smith's later scientific career can only be understood by noting such associations and recalling the date. He was thus in the mid-eighties taking a leading part in digesting and discussing sections of the European medical literature when it was under the formative influence of Pasteur, Lister, and Koch. There is record of Smith's comment that at this time he wished to be a doctor. But this, like his earlier inclination toward language, is chiefly significant as showing how fully he threw himself into the work in hand and how eagerly he met each new intellectual challenge.

A peculiar sequence of such challenges followed in the next year which combined to turn Smith's interests in parasitism and pathology from the diseases of animals to those of plants. The first of these came during the summer and autumn of 1885 with the occurrence of an unusually destructive disease of potato, the

leaf blight and tuber rot.<sup>2</sup> Smith, then transferring from the State College to his senior year as student at the University of Michigan, made an intensive study of this disease in field and laboratory. He accompanied this with a thorough review of the pertinent European literature, including the classical researches of deBary, and published a report of his findings. The second significant event occurred in August, 1885, when the American Association for the Advancement of Science met in Ann Arbor. Smith attended and was elected to membership. The Ann Arbor meeting is notable in the history of American plant pathology because of the report of a committee appointed the preceding year for "The Encouragement of Researches on the Health and Diseases of Plants." This committee there announced that it had aided in the establishment for this purpose of a "Section of Mycology" in the United States Department of Agriculture. This marked the beginning of the Federal program concerning plant pathology in which Smith himself was soon to become the essential research leader. To this work he devoted himself for the next forty years, continuing in active service until a few days before his death. A glance through the bibliography at the end of this memoir gives some concept of the continuity and variety of his contributions. Fortunately, five years before his death Smith consented to make a personal analysis and summary of his scientific work. This "Synopsis" of his researches has since remained exactly as he left it in manuscript form, known only to a few friends. It constitutes such a uniquely valuable review and interpretation of his own work that its publication in connection with this biographical sketch seemed of commanding importance. It has therefore been included as a later chapter of this memoir. The writer's contribution should therefore be accepted as an introduction to Smith's own "Synopsis." While in this he well summarizes his researches, he has not so clearly defined their relative significance. He naturally addressed it to readers presumably informed as to the historical development of parasitology and modestly refrained from comparisons of his

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<sup>2</sup> This is caused by a parasitic fungus, *Phytophthora infestans*. It was first known in Europe when, some forty years before, it had destroyed the potato crop of Ireland and thus led to the great famine.

own work with that of others. Space does not permit of adequately supplementing his account with the details often needed to justify such comparisons. Something must, however, be attempted in order that the non-professional reader may understand the nature and value of Smith's leadership in plant pathology.

When he began his work in Washington on September 20, 1886, the general nature of parasitism as exemplified in fungus diseases of plants had been established in Europe under the inspiring initiative of Anton deBary. Farlow had brought the deBary methods from Germany to Harvard a decade before. From his laboratory several capable younger men, trained for carrying on similar work, had already gone to other universities. The Farlow school was primarily concerned with questions of taxonomy and the life histories of the parasitic fungi. Meanwhile from Millardet's experimental work in France in the mid-eighties came methods for control of the mildews of grape and other plants by spraying with copper fungicides. These and other possibilities with specific remedial methods especially stimulated the beginnings of the Federal work at Washington.

Antedating the Federal work and supplementing that of the Farlow school was the leadership of Burrill at the University of Illinois. Beginning in the late seventies he, with his students, led in the study of the parasitic fungi and associated problems concerning plant diseases and their control. These brought him, about 1880, to one of the important advances of the decade in this field. He showed that the serious and perplexing fire blight disease of pear and apple is infectious and that the causal agent is a bacterium. These and some later reports from Italy and France concerning bacterial diseases of plants were, however, not accepted as valid by the leading European bacteriologists and mycologists, especially those of Germany and England. A related challenge came later in this decade from Holland when Adolf Mayer reported the mosaic disease of tobacco to be infectious. Since he could find no fungus he suspected a bacterial parasite, although he failed to demonstrate any such organism. We now know tobacco mosaic as a filterable virus disease. A full decade was needed, however, after Mayer's observations,

before our modern concept as to the character of these "mosaic" and similar virus diseases was even glimpsed.

This hasty sketch indicates the background essential for understanding the significance of Smith's own contributions during his first decade in Washington. He began his work about the time of Mayer's report upon tobacco mosaic. His best efforts during the first six years were devoted to what in his "Synopsis" he terms "one of the most difficult problems imaginable, to wit: a wide-spread destructive disease of peach orchards (The Yellows) in which no parasite was visible." He later adds that he turned after some years from these researches because "the problem appeared to me to be insoluble in our then state of knowledge. For that matter it has remained unsolved up to the present time." He here refers to the fundamental nature of the disease. Without understanding this Smith himself could not be satisfied. But his evidence, as outlined in his "Synopsis," was accepted as final by the growers with whom he worked and talked, as well as by the pathologists who read his detailed reports of progress. Much painstaking experimental work was needed to disprove certain earlier misconceptions as to the cause and possible control measures. On the other hand, his conclusions strengthened the grower's confidence that once established in the orchard it continued its destructive spread. But how? The only way he could pass it from tree to tree was by budding or grafting. This might account for its origin with nursery stock but it left questions unanswered as to its persistent spread through the orchard. Concerning control measures, however, the growers accepted his evidence. Those who had relied upon the futile cultural practices advised by some earlier investigators soon united their efforts in checking the destructive spread of the disease through commercial orchards by compulsory extermination.

American plant pathologists were at that time in process of developing their methodology for the most part with simpler problems. They generally recognized Smith's work with the yellows and related peach diseases as exemplifying the highest of ideals and standards for research in this field. Recent progress with his basic problem concerning the nature of these peach

diseases fully justifies his judgment in the "Synopsis" in 1922 as to the inherent difficulties involved. His closing comment was that "they are now generally thought to resemble mosaic diseases." It is here pertinent to summarize the evidence which has continued to accumulate since Smith's death in substantiation of his early work and later conclusions as to peach yellows. I may, however, well introduce this by recalling a remark made by Smith some forty years ago, soon after he had ceased active work with the peach problems. We were discussing the resemblance in symptoms of peach yellows and the yellows disease of china aster and of ragweed. His closing comment was to the effect that when we understand the cause of the yellows of china aster we may have the key to that of peach yellows.

Before Smith's death L. O. Kunkel<sup>3</sup> demonstrated that aster yellows is caused by a virus. Working since with peach yellows, he has found this also to be a virus disease similar to aster yellows but with characteristics even more difficult of determination. In each case the virus is transmitted by a specific insect vector, a leaf hopper. With aster yellows the relations are further complicated by the fact that the virus is infective only after an incubation period of several days in the insect's body. Kunkel's results to date suggest that an even longer incubation period may be necessary with peach yellows. Recent advances with these yellows diseases are thus cited as indicating the complexities of Smith's first major problem, the soundness of his early work, and the judgment shown in his later surmises.

In addition to Smith's devotion to peach yellows during his early years in Washington he mastered the European literature of mycology, especially as related to diseases in plants, as few Americans had done. His reviews and translations of this period

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<sup>3</sup> Dr. Kunkel's researches with these yellows diseases were begun at the Boyce Thompson Institute in 1924 and have later continued at the Rockefeller Institute, Princeton. Several publications have been made of which only two need here be cited:

KUNKEL, L. O. Studies on aster yellows. *Am. Jour. Bot.* 13: 646-705. 1926.

———. Insect transmission of peach yellows. *Contrib. Boyce Thompson Inst.* 5: 19-28. 1933.

coupled with the high standards of his own research publications had an immediate and persisting influence upon American investigational work in this field. This was fortunately supplemented by the brief but significant work of Thaxter (1888-1891) on plant diseases at the Connecticut Agricultural Experiment Station. There Thaxter used in his researches upon the potato scab disease the pure culture methods of the bacteriologist. Waite had brought these methods to Smith's attention when he went from Burrill's laboratory to the Federal staff in 1889. Smith's earlier contacts in Michigan with Board of Health problems led him to be further stimulated by his associations with Theobald Smith and Veranus A. Moore, then working next door in the Department of Agriculture on infectious diseases of animals.

This combination of circumstances prepared Smith in 1893 to follow up a "fascinating subject," as he calls it in his "Synopsis," by making the study of bacterial diseases of plants his major field of research for the rest of his life. In this new field he sought, from the beginning, to apply the highest bacteriological standards of the period. This was well exemplified in his series of monographic publications, which appeared during the next three years,<sup>4</sup> each dealing with a single bacterial pathogen. Here again the influence of Smith's intensive and brilliant researches was felt, not only as a standard for such scientific work throughout the Department of Agriculture, but now, with wider significance, setting the pace for the rapidly developing research programs in plant pathology in the several State institutions.

It was such developments that led in the late nineties to the most dramatic incident of Smith's life. In connection with these early researches upon bacterial diseases of plants, Smith with characteristic thoroughness undertook a painstaking study of the world's literature dealing with these diseases. As he states in his "Synopsis," he began the publication of reviews of such literature but soon discontinued the series because he found this earlier work "with a few exceptions not very exact or very con-

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<sup>4</sup> See citations in the bibliography concerning *Bacillus tracheiphilus*, 1895; *B. solanacearum*, 1896; *Pseudomonas campestris*, 1897.

vincing." A natural lag in recognition, especially in Europe, of the quite different order of Smith's own work in this new field was to be expected. Had this been shown merely by lack of attention it would doubtless have been accepted, for the time being, with little notice even by Smith himself. But when one of the leading European writers of bacteriological text-books, Professor Alfred Fischer of the University of Leipzig, not only denied the possibility of bacterial diseases of plants but published disparaging comments on the character of Smith's workmanship something was called for in direct reply. In the "Synopsis" Smith makes but brief reference to this published controversy with Fischer concerning the existence of bacterial diseases of plants, except for the comment that "it silenced all the critics and won over the doubting European public." The significance of this bitter polemic in the history of bacteriology of this period is, however, such that any reader technically interested may well glance through the original publications. As the citations listed in the bibliography will show, these appeared, seriatim, in the leading international journal of bacteriology. The effect was not only the prompter attention to and acceptance of Smith's world-leadership in this field of research, but also the stimulation of investigations concerning bacterial diseases of plants in other countries. Naturally it was the younger men, especially in Germany, who gave increasing attention to these studies.

While stressing thus Smith's contributions to work on virus and bacterial diseases, one should not overlook the evidence of his breadth of interest and influence. It was in this same decade, as recorded in his "Synopsis," that he first focused attention upon the obscure but highly important group of fungus parasites responsible for the *Fusarium* root rots, or "wilt diseases." Nor was this merely accidental. The broader problems were forced upon his attention because he found these fungus wilts occurring upon the same hosts as the bacterial diseases he was then studying, especially in the mustard and nightshade families. Smith here evidences the fact that he was an exceptionally keen field observer and diagnostician. His general paper <sup>5</sup> on soil infesta-

<sup>5</sup> See citation in the bibliography to his paper on "The fungous infestation of agricultural soils in the United States" published in 1899.

tion remains one of the prophetic writings in the field of plant pathology.

At about this date (February-June, 1899), having a half-year leave of absence from my Vermont University duties, I accepted Smith's cordial invitation to transfer my own work upon a bacterial plant disease to his Washington laboratory. This was then located on the second floor of an old brick residence on Thirteenth Street, south of the original administration building of the Department of Agriculture. Up to this time, Smith had worked essentially alone. He trusted no one's aid in any technical laboratory work and only reluctantly consented, after a fortnight's association, to permit me to wash his glassware along with my own. Here he drew the line, however, continuing to sterilize all his own containers and make his own culture media. Soon after that date he opened his research laboratory to younger associates. This is indicated by the names of co-authors of several publications beginning with 1904. This was due, in part, to the increasing diversity in the researches under way. In part it was necessitated by Smith's assumption of a new and great responsibility for the preparation of his monograph, "Bacteria in Relation to Plant Diseases." To appreciate the magnitude of this, the reader unfamiliar with the publication should examine the three quarto volumes, understanding that from first to last Smith attended personally to every detail, giving exacting supervision to whatever he could not himself do.

Soon after the appearance of the first volume of this monograph, Smith began the work upon crown gall, which, with the collaboration of younger associates, was to continue throughout the remaining twenty years of his life. Others had suggested the possibility of plant tumors as having some likeness to cancer in animals. Especial attention in such comparisons had previously been given to the club root disease, which causes tumorous overgrowths in the roots of cabbage, turnip, and other crucifers. This is caused by an ameboid organism, a parasitic slime-mold, which invades the host cells and stimulates them to such pathological overgrowths. But no one had heretofore made intensive comparative studies of such plant and animal "tumors." Anyone knowing Smith would realize how naturally he felt

forced to accept this challenge and how dominantly compelling was the urge, once begun, to follow such studies with the intensity which he did. Whatever else is said, all must recognize this as a timely thing that needed to be done. It is doubtful if anyone would, or perhaps could, in that decade have done it so well. In connection with this, Smith not only mastered to a remarkable degree the voluminous international literature pertinent to cancer but familiarized himself with animal cancer types and tissues.

Reference to Smith's "Synopsis" shows that when this was written (1922) he had not yet seen the evidence accumulating about the location of the crown gall bacteria. In the following year there appeared the independent, almost simultaneous publications of Riker <sup>6</sup> in this country and of Robinson and Walkden <sup>7</sup> in England. These workers showed that the bacteria were located in the intercellular spaces and gave to the mechanism of "secondary tumor" and "tumor strand" formation a new interpretation. They showed that the bacteria were also located inside vascular elements and injured cells. Later the bacteria were observed inside large surface cells that had discontinued cell division.

Perhaps it is idle to speculate upon what might have been the influence on Smith's work with these problems could these facts have been discovered a decade earlier. To the writer it seems probable that, while it would undoubtedly have modified his emphasis upon certain details of his analogy between "tumor strands" in animal and plant tumors, nevertheless, Smith would have persisted in the essential fact-finding phases of his work, just as others are continuing it today. While it might have spared him some arguments with certain animal pathologists, Smith's long and intensive comparative researches with these malignant growths in plant and animal tissues had already won world-wide attention to this subject among cancer-specialists. In

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<sup>6</sup> Riker, A. J. 1923a. Some relations of the crown-gall organism to its host tissue. *Jour. Agric. Research* 25: 119-132, illus.

———. 1923b. Some morphological responses of the host tissue to the crown-gall organism. *Jour. Agric. Research* 26: 425-435, illus.

<sup>7</sup> Robinson, W. and Walkden, H. 1923. A critical study of crown gall. *Ann. Bot.* 37: 299-324, illus.

1913 the American Medical Association awarded him its certificate of honor for his work on "Cancer in Plants." In 1925, only a little over a year before his death, in further recognition of his leadership in this field, he was elected President of the American Association for Cancer Research.

It seems fitting to couple with this specific recognition some consideration of the broader significance of Smith's contribution. Smith himself in all his researches was, subconsciously at least, attacking his problems as a comparative biologist. He was at once most intensive in his personal work and most inclusive in searching biological literature for whatever was pertinent in the work of others. Smith's persistent comparative studies of the diseased tissues in tumorous overgrowths of plant and animal tissues may, therefore, be rightly evaluated as modes of attack upon such fundamental problems in cellular pathology. No previous worker had so recognized the potential worth of the materials and techniques of plant pathology as contributing to the advancement of comparative pathology. It is not without significance in this connection that the Rockefeller Institute of Animal Pathology at Princeton has broadened its scope to include plant pathology. It is also gratifying that the initial problems there undertaken were in continuation of Smith's earlier work with peach yellows.

The final testimonials to Smith's lifelong leadership and winning personality came only about three months before he passed away. At that time the American Phytopathological Society gave a dinner in his honor, followed by addresses concerning the characteristics of the life and work of their honored guest. An engraved brochure was there presented to him. This consisted of a dedicatory statement (1) followed by summaries of the three addresses of the evening: that of Frederick V. Rand, his long-time research associate (2); that of the writer, L. R. Jones, speaking for plant pathology (3); and that of William H. Welch, speaking for human and animal pathology (4).

1. To Erwin Frink Smith, scientist, linguist, poet, friend, who for forty years has devoted his life's service to the broad field of pathology, in grateful appreciation we the members of the American Phytopathological Society dedicate this memorial.

2. What Robert Koch was to the early days of human and animal bacteriology, that and more have you meant to the bacteriology of plant diseases. Almost single-handed you saw it through those first years of speculation and skepticism to its present broad and solid position among the sister sciences.

In your scientific work and influence you have made an indelible impression not alone upon plant science or animal science but upon the whole field of experimental biology. And what is to me most vital and reassuring, through it all you have never for a moment lost sight of the humanities nor of the beautiful things of the mind and of the world without. May I therefore be permitted to add the personal tribute of one who for over fifteen years has worked under the inspiration of your guiding hand.

F. V. R.

3. For leadership in early study of peach yellows, most stimulating example of dogged work upon a baffling problem, with prophetic assurance that knowledge of tobacco mosaic and aster yellows was pertinent to the solution;

For leadership in pioneer studies of bacterial plant pathogens, with classic publications, exacting models for all who followed; again with prophetic vision of the boundless extent of this field;

For zealous devotion in defense of truth;

For assembled contributions to knowledge of bacteria in relation to disease in plants;

For epochal researches on crown gall;

For sympathetic counsel to eager younger scientists, from far and near;

For thus exemplifying the Pasteurian characteristics: clear vision, instant action, intuitive judgment, precise method, tireless endeavor, sympathetic patience, self-sacrificing devotion in service through science;

For these things we delight to honor you:

Pioneer, prophet, exemplar, dean of our Science.

L. R. J.

4. No one in our day has done more to bring the two great divisions of pathology into close relations to their mutual advantage.

Your studies of plant tumors have brought you into the field of onkology in its broadest aspect. Here you take your place in national and international congresses and associations devoted to medical research and here your work is recognized as of the greatest in interest and importance.

While your name is associated especially with the championship of the parasitic theory of the origin of tumors, your studies of the mechanism of tumor formation, of problems of histo-

genesis, of formative stimuli and inhibitions of growth are scarcely of less importance.

We too on the medical side have learned to admire you as a man inspired with the highest ideals of the searcher for truth, and devoted to this search, with the heart, the methods and the loyalty of the ideal man of science.

W. H. W.

The end came a little over three months later (April 6, 1927) at his home in Washington, D. C. The funeral services were conducted on Saturday morning, April 9, at All Souls Unitarian Church in Washington, the Rev. U. G. B. Pierce officiating. In accordance with the known wishes of Dr. Smith, his ashes were scattered over the waters at Woods Hole, Massachusetts, from a promontory where he had loved to sit and muse.

In closing this memoir, recognition should be made of Smith's ever-increasing devotion to the finer cultures of life. Frequent tributes to this may be found in the earlier biographies listed at the close. None of these has more truly glimpsed the complex of Smith's scientific and artistic genius than has that of Rodney H. True, his long-time associate in Federal service. With Doctor True's approval we may therefore well close with the following excerpt from his tribute.

After referring to Smith's breadth of interests outside his laboratory through which he "stroved with wonderful effectiveness to defend himself against the harmful results of specialization," True continues:

"He developed a knowledge of French, German and Italian literature that opened to him worlds of intense pleasure. Often have I seen him pursue some theme from language to language with an enthusiasm and facility that showed how deeply he read and thought. He read his Bible in a copy of the Vulgate; and Dante was a favorite with him in Dante's own great language. Goethe was often quoted in the original. Seldom have I known a man, whatever his training and field of work, who brought such joy and understanding to the works of great writers. His library was a sort of map of his mind. In it were all manner of noble things.

"His ear never ceased to find delight in music, more and more as the years went by, be it the music of the great poetry of the past or that poetry expressible in mighty harmonies. He took great delight in beautiful paintings, in sculpture and in architecture. No road along which beauty might enter was blocked.

"I think that while Dr. Smith was a true scientist to the very heart, he felt cramped by the physical world and sought greater freedom in the world of imagination where he could live as every man once in a while feels a desire to live . . . It seems to me that Dr. Smith was organized as artists, rather than scientists, are supposed to be. He was quick, enthusiastic, and strangely appealed to by beauty in all its forms . . . he may have had to learn the lesson of reserving judgment, of remaining skeptical, in short the whole defensive attitude of science. Thus the imagination of the artist was fundamental and by opening the book of nature it revealed to him the far reaches of life."

## Synopsis of Researches of Erwin F. Smith

In the United States Department of Agriculture (1886-1922) <sup>1</sup>

To summarize in a few thousand words the research work of thirty-five years, as you have requested, is something of an undertaking.

After a university training which today would be considered very inadequate, I began work in the United States Department of Agriculture in the autumn of 1886 (aet. 32) on one of the most difficult problems imaginable, to-wit: a wide-spread destructive disease of peach orchards (The Yellows) in which no parasite was visible. The losses in Maryland and Delaware, where thousands of acres of peach orchards were cultivated like gardens, were enormous and the work correspondingly urgent, and I entered into it with more enthusiasm than knowledge or good judgment. The results of this work are embodied in three big Department Bulletins and a half dozen or more shorter papers. After some years I abandoned this research and devoted my time to other subjects, mainly, as I have often said, to save my reputation, but really because the problem appeared to me to be insoluble in our then state of knowledge. For that matter it has remained unsolved up to the present time.

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<sup>1</sup> Shortly after the World War, Dr. W. B. Brierley, English mycologist, visited the United States, spending some time in Washington. During this time he was a frequent visitor to Dr. Smith's laboratory and home. Brierley highly esteemed Smith and his work and the feeling was mutual. During one of their friendly conferences Brierley asked him if he had assembled material for an autobiography. Smith replied in effect that no one would be interested in the story of his life, and as to his scientific contributions the published papers would have to speak for themselves. Brierley continued to urge that he owed it to himself and to the scientific world to do something of the sort. Though not fully persuaded, Smith, after further consideration, concluded that perhaps Brierley was right and prepared this "Synopsis." Soon after this was written Dr. Smith gave a typewritten copy of it to each of several associates. Among these was Frederick V. Rand, a long-time member of his staff, to whom he told the circumstances which led him to write it. It has never before been printed. We are indebted to Dr. Rand for furnishing from memory the essentials in this statement.—L. R. J.

I specialized in the university on Peronosporaceae and during the first years of my connection with the Department of Agriculture I spent a part of my time, especially one winter, on *Phytophthora infestans*. I mapped the distribution of potato rot in the United States for the year 1885, and that of *Plasmopora viticola* for 1886, but I gave most of my time to a study of peach yellows (1886-1892) and to peach rosette (1888-1891), two very destructive diseases of the peach now generally thought to resemble mosaic diseases.

In case of peach yellows, known for many years and frequently written upon by horticulturists and others, I described the signs from exhaustive observations, showed that it could not be due to winter injuries, nor to root aphides, one species of which I described as new (Ent. Am. 1890), redemonstrated spread by grafting and budding, showed that the disease could not be cut out of slightly affected trees, nor carried on seed from diseased trees, followed the progress of the disease in hundreds of new cases every year, and by many experiments with fertilizers showed that it could not be due to exhausted soils. I also moved a carload of Delaware soil taken from a badly diseased orchard to Central Michigan and buried it around the roots of many healthy peach trees but no disease resulted. In two dry summers alternating with two wet summers I showed that the cases were most numerous in the dry seasons (Journal of Mycology, Vol. VI). Some thought the disease associated with overbearing but I found that it often began in young trees, and that there were many new cases when there was no fruit, even two years or three years running. Orchard XV of my Bulletin No. 4 (Veg. Path.) may be taken as an example of the destructiveness of this disease in one of the bad districts of middle Delaware. This orchard which never bore heavily, and most years not at all, contained 3,000 trees set 20 feet apart each way, and the cases by years were as follows: 1887 (year trees were planted) 0; 1888, 0; 1889, 3; 1890, 144; 1891, 338; 1892 (dry summer), 1091. Thus in three years over one-half of the orchard, propagated from a neighboring nursery, became diseased. In those years in Maryland (Kent Co.) and Delaware (Kent Co.) I saw the disease destroy many large orchards in seven to ten years from planting,

whereas 50 miles away (Caroline Co., Md.) there were peach orchards 40 years old still entirely free from the disease but which subsequently became infected. I also got, when set into badly diseased orchards, what I considered to be a few undoubted cases in peaches worked on plum roots. The tops came from trees that were outside of the diseased area and that remained healthy.

In case of peach rosette, which is more southern in its distribution and swifter in its action and was then a new disease, I showed how it differs from yellows, proved that it could be spread in the same way by grafting or budding, established that sometimes it did not occur in one side of a tree when it was present in the other side although the following year the entire tree became diseased, showed that when root grafts were made the disease developed later on the top of the plant than when parts above ground were budded with the diseased buds, found numerous gum-pockets in the wood of the diseased roots (wood only of the season in which the disease developed) and in the shriveling fruits, and showed that mere contact of diseased tissues with wounds would not induce the disease but that some fragment of the grafted wood or bark must heal on in order to transmit the disease, although the tiniest bit was sufficient. There were 124 trees in my first budding experiment all but four of which contracted the disease. The signs of disease appeared first around the inserted diseased buds and a few months later the whole top became diseased. These trees stood in two nursery rows and none of the several thousand other nursery trees developed the disease. The cause of the disease was not determined.

While working on peach yellows in Michigan I discovered a peculiar disease of the peach, then new to the country but which has since attracted considerable attention, and published the first paper upon it. This disease is called "Little peach" because one of its conspicuous signs is the dwarfing of the fruits so that the trees become worthless. The cause of this disease also remains unknown.

During this period I published two papers on the brown rot of the peach (*Monilia*), one establishing its disastrous prevalence in the eastern United States in 1888 and proving that the

*Monilia* winters over in the mummied fruits, sporulates again abundantly the following spring in the form of conidia and causes blossom blight and canker of the shoots (Journal of Mycology, Vol. V). The second paper (Journal of Mycology, Vol. VII) gives additional details and figures the blight of the blossoms and the canker of the stems. It was not until some years later that Norton discovered the perfect form of this fungus (*Sclerotinia*) on mummied fruits two years old.

During this time I also studied gummosis of the peach and "foot rot" of the orange, with Comes' findings in mind, but without being able to convince myself that either one was due to bacteria.

In one of these years (1890) Mr. Walter T. Swingle and myself were sent posthaste into Florida to study a new and destructive disease of orange groves, characterized by the sudden wilting and shedding of the leaves. The disease appeared either first on certain branches only or all at once on nearly the whole tree. The smaller roots were clustered. Many fine orange trees in Central Florida were destroyed by this disease which was generally known as "the wilt." We came to no conclusion as to its cause other than that it appeared to be underground. What trees this disease spared the great freeze destroyed.

In 1892-3 I worked on an *Alternaria* disease of muskmelons which in Michigan I had seen destroy whole fields. The organism was cultivated from single spores and studied as to its morphology and behavior on culture media. I made many drawings and obtained numerous beautiful infections by spraying pure sporulating cultures on the foliage (Proc. A. A. A. S. for 1893, p. 258), but I never published *in extenso* because I found Victor Peglion in Italy had obtained the same results a year earlier and also, and chiefly, because I could not find any ascospore form of the fungus, which at that time I had thought must necessarily occur.

So closes the first period of my Department researches during which I did much proof-reading, reviewing, translating, and editorial and miscellaneous hack work.

Next I devoted a number of years (1894-1910) to the study of the *Fusarium* diseases of plants, a subject which was then

very new. There were at this time in the United States a number of destructive diseases of unknown origin, particularly on staple crops in the Southern States, in which I found *Fusaria* constantly and suspiciously present. I studied and made experiments with the *Fusaria* present in diseased melons, cotton, cow-peas, potatoes, tomatoes, and cabbage. I isolated the fungus from the interior of these plants in pure cultures derived usually from single conidia and with it produced the disease abundantly in case of several of them, thus showing it to be the parasite. I proved infections from the soil; inability of the various isolations to cross-inoculate, *e. g.*, on soil infected with pure cultures of the melon fungus I grew, from seed, rows of watermelons containing many plants, every one of which contracted the disease, alternating with rows of tomatoes, none of which contracted the disease although they were in the infected soil and only a few inches away from the dying melons; showed that the melon *Fusarium* was still infectious after being held dry in culture tubes for three years, and in case of the cabbage disease that the organism causing it remained alive and able to infect in soil from a diseased field which had been kept dry in the laboratory for two years. Special attention in a Department Bulletin on the potato *Fusarium*, published jointly with Deane B. Swingle,<sup>2</sup> was called to this black ring disease of the potato tuber which was then a new disease, at least to scientific men, and while infections were not undertaken the *Fusarium* was demonstrated to be the only organism constantly present in the diseased blackened vascular bundles and in the light of the many successful inoculations previously obtained on melon it was stated to be the parasite (a conclusion since confirmed by the experiments of others). This first good paper on this widely prevalent potato disease (Bureau of Plant Industry Bulletin 55, 1904) is now seldom referred to because

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<sup>2</sup> In 1904 I made with Deane B. Swingle more than 100 freezings of various bacteria in liquid air, and in salt and crushed ice, showing that the critical temperature is around zero Centigrade and that repeated short freezings and thawings are much more destructive than a single longer freezing (Science, March 31, 1905, pp. 481-483). These results were confirmed in 1918 by Hilliard and Davis (Jour. of Bacteriology, pp. 423-431) but no mention was made of our work.

the writer unfortunately resurrected an old name, *Fusarium oxysporum* Schlechtendal, which was practically a *nomen nudum*, and applied it to this fungus.

As a result of these studies, which opened up a new field of plant parasitism, since all members of the form-genus *Fusarium* had previously been considered to be pure saprophytes, the writer read a paper "On the Fungous Infestation of Agricultural Soils in the United States" at a meeting of the American Association for the Advancement of Science in August 1899, which was soon afterwards published (November, 1899) in the *Scientific American Supplement*, No. 1246, pages 19981-82, and was the first paper on this subject published in the United States, or anywhere, so far as concerns soil infections due to *Fusariums*, some of which are as destructive as *Peronosporas*. In 1910 the writer showed a destructive West Indian banana disease to be due to a *Fusarium* (*F. cubense* EFS.) and this conclusion has since been confirmed and the disease studied exhaustively by Brandes (*Phytopathology*, Vol. IX). This soil disease, probably the worst banana disease in the world, certainly the worst in the Western hemisphere, has put thousands of acres out of commission in Central America and in Dutch Guiana in recent years. Dr. Ernst Gäumann (On a vascular bacterial disease of the banana in the Dutch East Indies, No. 48 Med. v. h. Institut voor Plantenziekten, Batavia, 1921) has recently called these conclusions in question but without any first-hand knowledge of the West Indian disease. Through the work of W. A. Orton, whom the writer selected for this purpose, highly resistant cottons, melons and cowpeas have been obtained and are now growing on *Fusarium* infested soils in the South.

During this period I wrote the mycological and plant pathological definitions of the first edition of *The Standard Dictionary* (with Walter T. Swingle from D to G and through the remainder of the alphabet alone)—a task of midnight hours.

In 1893, having found a fascinating subject in the cucumber wilt, I became especially interested in bacterial diseases of plants, then a rather new field, full of obscurities, and have done original researches on such diseases every year from that time to the present, having undertaken to monograph the whole group.

During these researches I proved water-pore infections in case of black-rot of cabbage; stomatal infections in half a dozen diseases; and insect transmission in cucurbit wilt, black-rot of cabbage and brown-rot of potato. Merton B. Waite, one of my colleagues, had earlier (1891) proved infection of the floral nectaries by bees and other insects in pear blight due to *Bacillus amylovorus*, and one of my assistants, Dr. Frederick V. Rand, has shown recently not only that my statements respecting summer transmission of cucurbit wilt by *Diabrotica vittata* are correct but also that my suspicions of its being a winter carrier of the bacillus (*Bacteria in Relation to Plant Diseases*, Vol. II, p. 215) were well founded. What occurs in some human diseases occurs also in case of this beetle. It is a carrier of disease and an intermediate host. It feeds greedily on the wilted leaves which are full of the bacillus and then gnaws healthy leaves to which the bacillus and the disease are transmitted. In most of the beetles this is the extent of their complicity, their intestinal contents soon destroying the ingested bacillus; but in others the bacillus multiplies and persists in their digestive tract during hibernation, i.e., until the following spring when it is voided in their feces and produces on gnawed or otherwise wounded leaves of susceptible plants the first spring infections (*Phytopathology*, Vol. X, pp. 133-140).

Urged on by Robert Hartig's and Alfred Fischer's denials, I also made many experiments with the olive tubercle using pure cultures plated from tubercles obtained in Italy and in California, repeating and confirming the positive inoculation experiments of Savastano and Cavara and for the first time demonstrating the morphology and cultural characters of the parasite never clearly expressed by the early workers and subsequently brought into much confusion by others.

Another result of these researches was the discovery of the bacterial nature of a whole group of tumors of uncertain origin, commonly known as crown galls. Associated with me in this discovery were Dr. C. O. Townsend and Nellie A. Brown. The results of this work were several papers in journals (*Science*, *Botanical Gazette*, *Phytopathology* [Vol. I], *Centralblatt f. Bakt.*,

etc.) and Bulletin 213 from the Bureau of Plant Industry, United States Department of Agriculture.

A critical review of the literature of bacterial diseases of plants was begun in 1896 in the *American Naturalist* but after a few numbers was broken off owing to pressure of researches which suddenly developed and appeared to me to be much more important, particularly as the literature of that time, with a few exceptions, was not very exact or very convincing.

In 1899-1901 occurred my controversy with Dr. Alfred Fischer as to the existence of bacterial diseases of plants (*Centralb. f. Bakt. 2te. Abt.*). This silenced all the critics and won over the doubting European public. Fischer never forgave me, but I could not do otherwise; nor do I regret the polemic, since it cleared the air and advanced the science.

In 1903 "Bacteria in Relation to Plant Diseases," a monograph, with many plates and text figures, was projected with permission of James Wilson, Secretary of Agriculture, and undertaken in a very liberal spirit by the Carnegie Institution of Washington. Of this monograph three quarto volumes have been published (1905, 1911, 1914) and three more will be required to complete the project. But even if it is not completed I have written many papers on various diseases, a description of which would naturally form part of the concluding volumes, e. g., on blights and tumor diseases, and many good workers are now in the field so that there is not that urgent need for its completion there was for its beginning. I hope, however, to complete at least Vol. IV, the manuscript of which is now well in hand.

Such urgent requests came to me from teachers that "Bacterial Diseases of Plants" (Wm. B. Saunders Co., Philadelphia and London), a text-book with numerous hitherto unpublished observations and 650 illustrations, was published in 1920. The difference between the title of this book and that of the Carnegie monograph shows very well the changed attitude of the public to which the writer conformed, changing from a title of argument and persuasion to one of certainty. This is the first handbook on bacterial diseases of plants.

In 1920 also, and by the same firm, was published "Pasteur, The History of a Mind," the same being a translation of Émile

Duclaux's fascinating account of the development of the scientific spirit in Pasteur. This translation was made so that many young English-speaking laboratory men and women to whom the French text is not accessible might have it in their own tongue, since it is as fascinating as a novel and splendidly emphasizes right methods of work without which there can be no progress in science. Florence Hedges, one of my assistants, was associated with me in its preparation.

Fortunately, I determined from the beginning of my studies of bacterial plant parasites to depend for proof only on experimental evidence derived from pure culture inoculations, and consequently I cannot recall that I have ever asserted a disease to be due to bacteria which was subsequently shown to be due to some other cause unless it be in case of coconut bud rot (Science, March 31, 1905), which is still in dispute, and where my sole dependence was on the microscope and the result of many poured plate cultures made from the advancing margin of diseased tissues. Johnston who worked on it independently came to the same conclusion. Reinking maintains that the primary cause of the Philippine bud rot of coconut is a *Phytophthora*, but whatever may be the wounding cause, bird, insect, or fungus, I must still think the West Indian bud rot of the coconut as I saw it at Baracoa and Mata in 1904 is due to bacteria, but of course there may be two or more bud rots.

The microscope alone in the hands of an expert pathologist and bacteriologist will often yield fairly conclusive evidence as to the parasitism of an organism but in the hands of a tyro it has often led astray. Hence the utility of the three famous rules of proof devised by Robert Koch, to which the writer added a fourth—reinoculations from the reisolutions with positive results, again and again. (See *Bacteria in Relation to Plant Diseases*, Vol. I, pp. 9-17.)

In recent years so many of these diseases have come to my attention that I must suppose there are many others still undescribed in various parts of the world, and that, as I asserted in 1896 in a time of great scepticism, "there are as many bacterial diseases of plants as of animals" (*Am. Nat.* 1896, p. 627), only

then I added "in all probability", whereas now we may leave out that qualifying phrase.

Of these bacterial diseases of plants the following may be mentioned as having absorbed a good deal of my time for thirty years and on all of which I have published notes, or one or more papers, in many cases several papers.

I. *Wilt of cucurbits* (1893-1911) due to *Bacillus tracheiphilus* EFS. This is rather widely distributed in the United States and occurs also in Europe, Africa, and Asia. It attacks, wilts and shrivels cucumbers, muskmelons, pumpkins, squashes and some wild plants. The watermelon is resistant. It is a vascular, wound-infection disease which occurs in the forcing house as well as in the field and is distributed especially by leaf-eating beetles (*Diabrotica vittata* and *D. duodecempunctata*). The organism is not evident on the surface of the plant even in late stages of the disease but the spiral vessels and tracheids are then filled with its white slime which is generally very viscid, stretching out in long, delicate threads. It also disorganizes the primary vessel parenchyma forming numerous bacterial cavities. The organism is killed quickly by drying and there is a strain which attacks cucumbers and not squashes.

II. *Brown rot of Solanaceae* (1895-1920) due to *Bacterium solanacearum* EFS. I first described this disease from tomatoes, potatoes and egg plants and subsequently from tobacco (1908) but, thanks to Honing, Wolf, and others, it is now known to attack plants of various families in the East Indies and elsewhere and I have myself worked with it on Euphorbiaceae (*Ricinus*), Onagraceae (*Fuchsia*), Leguminosae (beans and peanuts), and Compositae (*Helianthus*), etc. The organism is a polar flagellate white rod which, however, produces a brown to black stain on various media (agar, steamed potato) and in many of the host plants. It not only attacks and destroys the cultivated plants I have named but occurs on or may be inoculated into various weeds of several families, e. g., *Daturas*, *Ambrosia*, *Eclipta*. On potatoes it attacks and shrivels the shoots and travels down the vascular system into the tubers, the brown rot always appearing first in the vascular ring of the latter and only later coming to the surface. In this disease there is eventually more or less

slimy brownish ooze to the surface of the plants, accompanied by dark leaf and stem stripes which are the stained vascular bundles showing through the uninjured outer tissues. The organism persists in certain soils and tomatoes and tobaccos are often infected from the soil through roots broken at the time of transplanting. The disease often follows the labors of a careless planter like a pest but respects the work of a careful man. The disease is more southern in its distribution than the preceding. It occurs in many parts of the middle and southern United States, in the West Indies, in Africa and Asia, and probably in South America. It is very common in the Dutch East Indies and in Japan on tobacco. Warm temperatures are necessary for its rapid development. Under favorable conditions pith and cortex are honeycombed with bacterial cavities. The organism, which is non-liquefying except perhaps slightly in prolonged culture at high temperatures (Nakata), loses virulence readily on media and is sensitive to dry air. There is a strain which splits fat with production of an acid.

III. *Black rot of crucifers* (1897-1911) due to *Bacterium campestre* (Pammel) EFS. Pammel's work on turnips was extended to cabbages, cauliflowers, kale, rape, mustard, etc., with many additions and much field, laboratory and hothouse work. The disease is due to a yellow, polar flagellate, liquefying, starch-destroying organism, which enters through the water pores or through wounds, but seldom through the roots, and multiplies very abundantly in the xylem part of the vascular bundles which take on a dark stain so that usually the attacked leaves of cabbage show a black net-work on a pale green or yellowish ground. There is little ooze to the surface and the disease is not a wet rot, but soft rots may follow it. All cultivated crucifers are subject to it and the organism causing it is carried on the seed. The disease has been widespread and destructive in various parts of the United States and occurs in Europe, as Harding first showed, and in other parts of the world. I have seen it in the West Indies.

IV. *The yellow disease of hyacinths* (1897-1911) due to *Bacterium hyacinthi* Wakker. This disease, apparently confined to the Netherlands, was first studied by the writer on Dutch bulbs

in the United States and afterwards in Holland, Wakker's statements, very good for their time, being confirmed and extended, especially as regards the biology of the parasite, sensitiveness of varieties, manner of infection, etc. A few varieties are very resistant and have retained the resistance for many years; others, which were noted by Wakker as much subject to the disease are still subject or have been discarded, while still others formerly recorded as resistant are now attacked. The organism causing the disease produces longitudinal stripes on the leaves and a bright yellow slime which oozes from the cut vessels of the bulb. It is a feeble liquefier, has less action on potato starch than the preceding or the following, and its action on the hyacinth is slow. It is principally a vascular disease.

V. *Bean blight* (1897-1920) due to *Bacterium phascoli* EFS. This is a widespread disease in the United States both on bush beans and climbing sorts and often does much damage. It occurs also in Europe, Asia and Africa. The disease attacks leaves, stems and pods. It is especially a disease of warm seasons and moist weather. It is distributed on the seed and stomatal infections are common and are very easily obtained in the hothouse or field by spraying, especially if the soil and air are moist and the temperature is high. It is sometimes found in the vessels but grows most abundantly in the intercellular spaces forming bacterial pockets in the parenchyma and oozing to the surface freely as a yellow slime, drying in crusts. The organism is yellow on media, polar flagellate, starch destroying and much like No. III of this paper, but does not cross-inoculate.

VI. *Cobb's disease of sugar cane* (1901-1914) due to *Bacterium vascularum* (Cobb) Greig Smith. I verified Cobb's conclusions and extended them on cultural material isolated from diseased canes received from New South Wales. I obtained the first clean cut pure culture inoculations in a hothouse in Washington in 1903, and subsequently worked out the cultural characters of the parasite. The signs of the disease are dwarfing, white or yellow striping of the leaves, gumming of the upper leaves with twisting and kinking of the terminal bud, death of the growing point, with appearance of yellow slime in the bundles of leaves and stems. Many of the bundles are also stained red

but this is a host reaction not confined to Cobb's disease. This is a vascular disease, but in the softer parts especially under the terminal bud the parenchyma breaks down into large cavities full of bacteria and fragments of tissue. In my inoculation experiments, which covered a period of four years, some varieties of cane proved much more susceptible than others. The organism is polar flagellate and yellow on various culture media and belongs with the yellow species already mentioned. The disease occurs injuriously in Australia (Queensland and New South Wales) and probably in other cane growing regions of the world but I have not seen typical specimens from either North or South America, and there is some doubt as to other cane regions. The disease is spread in cane cuttings.

VII. *Stewart's disease of maize* (1898-1920) due to *Aplanobacter Stewarti* (EFS) McCulloch. This is a common disease especially on sweet maize in the warmer parts of the United States but it is found also on some varieties of field maize. It occurs also in South Africa and probably in Australia. High temperatures and abundant moisture have much to do with its prevalence. It is much more common on early and choice sweet sorts than on later and coarser kinds. The disease is transmitted on the seed but often it does not appear in the field conspicuously till one or two months after planting. The writer obtained the first convincing infections with pure cultures in 1902, making his inoculations by placing the organism on the tips of leaves extruding water, i. e., in the seedling stage and transplanting to the field some weeks later. The infections are stomatal and probably also by way of the water pores. Dry and cool conditions are very unfavorable to general infection of the plant. When this occurs the plant is dwarfed, the leaves shrivel one after another from below upward, the ears are more or less abortive, the male inflorescence develops prematurely, and on cross section of stems there is an abundant ooze of yellow slime from many vascular bundles. In the husks the bacterial slime forms many small cavities and oozes to the surface freely through stomata, often covering the kernels, but I have found it also in the vascular bundles at the base of the kernels. The writer first described the parasite from cultures sent to him by Stewart for

that purpose and figured it as polar flagellate, but subsequent studies in his laboratory showed this to be an error and the statements in the text-book are more dependable as to morphology and cultural characters than those in Volume III of the monograph. The organism is non-motile (Lucia McCulloch) and only feebly active on potato starch. It attacks leaves, stems, husks, cobs and kernels, and in the latter it persists in a viable condition for a considerable period—more than a year. There are various yellow saprophytic bacteria on the kernels of maize.

VIII. *Mulberry blight* (1905-1921) due to *Bacterium mori* B. and L. emend, EFS. In 1893 Boyer and Lambert published a note on this disease in C. R. of the French Academy and claimed infections with a schizomycete isolated from the diseased tissues. They promised another paper in which they would give details and describe the organism, but never published anything more. In Italy various persons have studied a bacterial blight of the mulberry and described a yellow organism as its cause, but without any clear cut infection experiments. I began work on the American mulberry blight in 1905 but owing to Italian statements the first summer was devoted to yellow schizomycetes which are not infrequent. As none of many inoculations with yellow organisms plated from the diseased tissues gave any positive results I turned my attention thereafter (1908) to a white, non-sporiferous, non-liquefying organism common in the diseased tissues and with it have obtained many good infections, in different years, the last of which were in 1921. I assumed from the beginning that the American disease was the same as the French, an assumption since confirmed by my own experiments and those of Arnaud and the only doubt that now remains is as to whether there may be another bacterial disease of mulberry in Italy due to the yellow sporiferous organism, *Bacillus cubonianus* Macch. This I will not undertake to decide. As Boyer and Lambert obtained infections with the organism they called *Bacterium mori* I think, as I have stated elsewhere, that we may use their name and with a nearly clean slate write a proper description of the organism. This at least is what I have done.

IX. *The black spot and canker of peach and plum* (1902-1922), due to *Bacterium pruni* EFS. I discovered this disease on Japanese plums in the United States (Michigan) but it occurs also in Japan and in both countries attacks both plums and peaches. In the United States the disease occurs from New England to Georgia and westward to beyond the Mississippi. The disease is not a soft rot, but one of meristematic tissues. Old tissues cannot be infected. The signs of the disease are spots on leaves and fruits and cankers on stems which latter carry the disease over winter. In bad seasons the trees are defoliated in late summer and the spots on the fruits render them unsalable. The infections are stomatal. The spots at first are very small, circular and watersoaked in appearance, but they slowly enlarge, coalesce and shrivel so that on the green fruits of the plum there may be numerous round or irregular black sunken spots  $\frac{1}{4}$  inch or less in diameter. European varieties are less subject than Japanese. Generally on peach fruits the spots are smaller but they may be numerous. Often as they approach maturity the spotted fruits crack open inviting the entrance of various fungi. On leaves the shriveled spots drop out easily and the tissue between spots often becomes yellow. On branches also the progress of the disease is slow with formation of numerous small bacterial pockets in the cortex ending by fusion in rough cracks, but even near such pockets the tissues for the most part appear to be alive or to die slowly. I have produced the disease with pure cultures in different years on fruits, leaves and shoots of the plum and on leaves and stems of the peach and have published various notes on the disease in Science and elsewhere and good figures in my monograph (Vols. I, II). I have not yet given a full account of the organism but Rolfs has done so (Mem. 8, Cornell Univ. Agr. Exp. Sta., 1915). It is a yellow polar flagellate species growing readily on a variety of common culture media. Gelatin is liquefied, nitrates are not reduced, and milk is slowly coagulated (lab) and digested with formation of tyrosin, etc. It is easily inoculated through wounds or by spraying. Generally it can be detected on thin sown agar poured plates by the appearance of its colonies which are round and pale yellow, smooth on the

surface but with internal striae visible by oblique light. The organism is not viscid when grown in Uschinsky's solution but is often accompanied by a yellow saprophyte which makes that culture medium extremely viscid so as to resemble white of egg (Bacteria in Relation to Plant Diseases, Vol. I, fig. 11). In young shoots of Japanese plum inoculated on one side only by needle pricks, I have observed that the inoculated side ages faster than the opposite side as shown by the premature formation of lenticels and of cork. In one orchard I observed the west side of fruits to be ten times as spotted as the east side and Halsted observed the same thing in case of the bean blight (No. V). This I attribute to persistence, on the side opposite the morning sun, of rain or dew favoring infection. In one orchard I observed trees on the moister land to be most subject to attack and Kuwatsuka has shown (An. Phytop. Soc. Japan, Vol. I) that wet soil is correlated with wide open stomata and many infections.

X. *The angular leaf spot of cotton* (1900-1920) due to *Bacterium malvacearum* EFS. The name angular leaf spot was given by Atkinson to indicate the limiting action of the large veinlets. He observed bacteria in the spots but his infections were unsuccessful. I cultivated and described the organism, first reproduced the disease by pure culture inoculations and also first demonstrated that the black arm of stems and the rot of bolls are a part of the same disease. I showed also that the cotton gummosis of Asia Minor and the leafspot of South Africa are due to it. We now know that the disease occurs in all the principal cotton growing regions of the world and that beyond much doubt it is commonly distributed on seed. Cook has seen it in China but chiefly on Egyptian cotton and Bovell and Dash have reported it as serious on late cottons in the Barbadoes. It is due to a yellow polar flagellate organism. The infections are stomatal and all parts of the cotton plant are attacked. The disease is much worse in some seasons and in some localities than in others. Many varieties are subject to attack but in different degrees. The disease also appears to pave the way for fungous infections particularly the Colletotrichum rot of the bolls. In severe cases the bolls drop off, or

become one sided with stained lint, the leaves fall early, fewer bolls mature, and the smaller branches break over. In other and perhaps the majority of cases the disease passes almost unnoticed. In rainy weather Faulwetter has shown that the disease may be wind-driven. Good figures with some account of the organism and of the literature will be found in my text-book.

XI. *The angular leaf spot of cucumber* (1906-1915) due to *Bacterium lachrymans* Smith and Bryan. This is a disease confined principally to the foliage which may be severely injured so that the crop is reduced in value or destroyed. As in the preceding the larger veinlets exert a limiting action on the spread of the organism in the tissues. The specific name *lachrymans* refers to the tendency of the parasite to ooze to the undersurface of the spots in copious fluid drops which dry to white crusts. The disease is common in the United States and probably occurs on the cucumber in all parts of the world. The infections are stomatal and the disease is not a soft rot. It is believed to be seed borne. The organism is a slowly-liquefying, white, polar flagellate, capsulate species.

XII. *Stripe disease of broom corn and sorghum* (1904-1922) due to *Bacterium andropogoni* EFS. This produces long red stripes on leaves and stems with copious ooze of the bacteria to the surface drying in reddish crusts so that the bacteria might seem to be red, but the red color is only a copious host reaction and the organism on media is white on agar poured plates, forming small, circular, slow growing, smooth, shining, more or less viscid colonies. The parasite, which is polar flagellate, aërobic, non-sporiferous, non-liquefying and non-nitrate reducing, enters by way of the stomata. It blues litmus milk and has slight action on potato starch. I secured many good infections on broom corn in one of our houses by spraying on the pure cultures diluted with sterile water and some good figures, including pure culture stomatal infections, have been published (Bact. in Rel. Pl. Diseases, Vols. I and II) but I have not yet fully described the organism nor have I seen it in recent years.

XIII. *The bacterial canker of tomato* (1909-1922) due to *Aplanobacter michiganense* EFS. This disease was first studied on material received from Grand Rapids, Michigan, and for

want of a better name was dubbed "The Grand Rapids disease." Subsequently in my text-book I gave it the above name. In large fields about Grand Rapids where tomatoes were grown for canning it proved a very bad disease. I have since received it from New York and Massachusetts and believe it is not confined to the United States. It is a slower disease than the brown rot of Solanaceae (No. II), one leaflet after another wilting and shrivelling rather than all at once. The infections are stomatal and the disease is probably seed borne. The parasite is frequently in the fruits in great numbers and I have seen it in the placenta close to the seeds but not actually in the seeds. To find it *on* or *in* the seeds is undoubtedly only a matter of time. The phloem and other tissues are disorganized and the organism shows a strong tendency to ooze to the surface through fissures in stems, leaves and fruits, the tissues being swollen and whitish before they crack open. The organism is yellow and non-motile. It cannot be plated from the plant unless the media are adapted to its growth. If the agar is too acid or too alkaline no colonies of the right organism will appear. In this connection consult figure 160 of my text-book. It is often accompanied by motile yellow saprophytes but I have never observed the parasite itself to possess any motility nor have we been able to demonstrate any flagella by any of our expert methods, and our non-motile organism continues to be infectious (1922).

XIV. *Lilac blight* (1906-1907) due to *Bacterium syringae* (Van Hall) EFS. Beyerinck isolated the organism and with it reproduced the disease on lilacs in 1899 and 1900. In 1901 Van Hall, using Beyerinck's old cultures, failed to obtain infections so that the personnel at the laboratory in Amsterdam were all sceptical when I began my investigations. I studied the disease in 1906 in a lilac nursery at Naarden. It spots and twists the leaves and runs up and down the young shoots as black stripes often more on one side than the other with distortions, but in bad cases the whole shoot is killed and many cavities full of bacteria are found in the cortex. The parasite, a polar flagellate, green fluorescent species, was isolated in poured plates in Amsterdam at the Willy Commelin Scholten laboratory and the disease was reproduced by needle prick inoculations on

healthy lilac shoots in the garden connected with the Institute. Although I considered the season too far advanced for best results by carefully selecting my shoots I obtained a number of good infections as all knew. In the spring of 1907 I repeated the inoculation experiments on lilacs in Washington with positive results and made some further studies of the organism but did not publish. The organism loses virulence on culture media as Beyerinck and Van Hall first showed. The disease occurs in England, Holland and Germany but I have not seen it in the United States except as I reproduced it here by pure culture inoculations. Güssow published on it in the *Gardeners Chronicle* in December, 1908.

XV. *Rathay's Disease of orchard grass* (1913) due to *Aplanobacter rathayi* EFS. In 1899 Emerich Rathay of Klosterneuberg, near Vienna, published a preliminary paper on "a bacteriosis of *Dactylis glomerata*" which interested me tremendously because it appeared to be founded on a new type of bacterial disease. At the time of his death it was known that he had completed the manuscript of a much more extensive paper covering many of the gaps in his first paper but unfortunately this was never published nor is it now in existence. In 1913 I received a disease of orchard grass from Denmark which corresponded in many ways to Rathay's description and the same year I visited Klosterneuberg and saw and photographed alcoholic material preserved by Rathay and also found a little of the disease in the woods on the Kahlenberg where he had collected his specimens. From this fragmentary material I made the note published in *Bacteria in Relation to Plant Diseases*, Vol. III.

The organism, like *Ap. michiganense*, is sensitive to culture media. The growth is chiefly external, i. e., between appressed parts of the spikelets and upper leaf sheaths and the gumming together of the latter leads to curious knee-shaped bucklings of the stems. No one has reproduced the disease by inoculations and the diseased clumps which I collected on the Kahlenberg and planted in one of our hothouses gave no diseased shoots the following years, possibly because our hothouse conditions were too dry. Since my notes were published, O'Gara has

found a very similar disease on Agropyron in Utah and Hutchinson on wheat heads in Punjab in India.

XVI. *Black chaff of wheat* (1915-1922) due to *Bacterium translucens* var. *undulosum* S., J. and R. This disease came to my attention in 1915. It first attracted general attention that year only in Kansas but we now know that it occurs in all or nearly all of the wheat states beyond the Mississippi. In 1917 I sent several collectors into the Middle West and corresponded with many persons in an effort to learn its distribution and prevalence. Every specimen sent in was numbered and examined and if hopeful cultured from, and in this way we found the disease many times on spring or winter wheat in fourteen states. Along with the field work, laboratory and hothouse and field experiments were undertaken in Washington to determine the cause of the disease. Jones and Reddy also worked on it independently in the West. It is due to a yellow polar-flagellate schizomycete easy to isolate and grow on a variety of media. It infects all parts of the plant beginning with the seedling. It produces water soaked and yellow or brownish stripes on the leaves and sunken black stripes on culms and glumes. It is carried over winter on the kernels; it occurs on their surface as dry films and in bacterial cavities inside of the grains. In severe cases the heads are shortened and the kernels are shriveled. Badly attacked fields have a brownish rather than a golden appearance as the harvest approaches. The bacteria ooze freely from the lesions and dry as tiny films or crusts on leaves, culms, glumes, awns, and berries. From cross sections of diseased leaves, glumes, or kernels mounted in water under the microscope clouds of bacteria may be seen to ooze, if examinations are made at once. The disease has been reproduced by pure culture inoculations in Washington by the writer and by various assistants many times during the last few years. Infections on the leaves are stomatal and probably also through the terminal group of water pores. In the field the disease is a variable one depending largely on weather conditions. Moist seasons favor it and dry ones check it. I have not seen it east of Western Illinois. Its sudden prevalence throughout that region of the United States which received many Russian wheats in recent years makes me

think that it was imported from Eastern Europe. I suspect that the disease occurs also in South Africa. It can be controlled by formalin seed treatment without injury to germination if the presoak method developed by Harry Braun of my laboratory is used.

The organism is closely related to *Bacterium translucens*, the cause of a barley disease. The specific name *undulosum* refers to the fact that on agar poured plates the surface colonies examined by oblique light are seen to be conspicuously internally striate although perfectly smooth by reflected or direct transmitted light (see fig. 13 and other figures of my text-book). Nothing is known as to its effect on flour.

XVII. *Olive tubercle* (1903-1922) due to *Bacterium Savastanoi* EFS. In 1887-9 at Naples, Luigi Savastano made for the time excellent researches on this disease. He found bacterial pockets in the tumors and reproduced the disease with a schizomycete isolated therefrom. Cavara repeated and confirmed Savastano's experiments but neither one gave any good account of the organism which was generally called *Bact. oleae* (Arcangeli) and considered to be yellow, till Schiff-Giorgini stated it to be a white, spore-bearing schizomycete. This species he described very fully but without any attempt at careful infections assuming other authors to have sufficiently established its pathogenic nature. Meantime Robert Hartig and Alfred Fischer both discussed the disease denying that any one had established its bacterial origin. In 1903 and subsequently I produced olive tubercles readily in our hothouses using (1) a non-sporiferous white organism isolated from olive tubercles obtained in California and again (2) using the same organism but plated from olive tubercles collected in Italy. Soon after, I obtained Schiff's organism and repeated most of his cultural experiments finding his statements in general to be correct including sporulation, but with his organism, which was peritrichiate flagellate and had all the characteristics of a common potato bacillus such as *Bacillus vulgaris*, the most painstaking and copious inoculations failed to produce any tubercles even on very young and rapidly growing olive shoots. Inasmuch as Arcangeli's name (*Bacterium oleae*) was given without any cultural characters and without

any evidence as to its pathogenicity or any belief in it, and as his organism was considered by Berlese to be a yellow species and by Schiff-Giorgini to be a white spore-bearing species, I gave to the very different polar flagellate, non-sporiferous organism which I had isolated in pure culture and had proved to be actively pathogenic a new name in honor of Savastano, the man who first showed the tubercle to be due to bacteria. The right organism is sensitive to heat, grows freely in Cohn's solution, blues litmus milk without coagulating it and forms typical, erose margined, non-liquefying white colonies on gelatin plates.

The disease occurs all around the Mediterranean and probably wherever olives are grown. The organism causing it commonly enters through wounds, and in my experiments the first evidences of overgrowth followed within a week or two of the needle pricks while under favorable conditions growth continued for a number of months. Cavities full of bacteria are formed in the tubercles, and the organism is able to invade by way of the spiral vessels with production of new tumors at a distance from the mother tumor as Schiff-Giorgini first showed. It also oozes readily to the surface of the galls in wet weather (Horne) and again enters the plant through wounds. Petri, who has verified many of my statements, claims to have established a symbiotic relationship with an olive insect, *Dacus oleae*, the digestive diverticula of which are full of bacteria. Some varieties of olives are more subject than others. The trees are dwarfed and rendered unfruitful but are seldom killed outright. The tubercle may be either hard and woody or quite soft and cheesy from excess of bark parenchyma but always it contains bacterial cavities more or less brown stained and with water soaked margins. The spiral vessels in which I found invasion of the bacteria with production of secondary tumors are also more or less disorganized and stained brownish. In old tubercles yellow and white saprophytic schizomycetes are common. James Birch Rorer was associated with me in my first studies of this organism.

XVIII. *The sugar beet tubercle* (1910-1911) due to *Bacterium beticolum* S., B. and T. This disease which somewhat resembles the preceding was discovered on sugar beets during

our study of crown gall and was reported on briefly in Bull. 213 (pp. 194-195, pl. XXXIV). The disease came to us from Colorado and Kansas in 1910 and has not been seen since. These beets had a conspicuous nodular growth superficially resembling crown gall, but in the center of the nodules there were areas of softening and cavities with water soaked brownish margins and a mucilaginous stringy content containing great numbers of bacteria. Pure cultures of a yellow, capsulate, polar-flagellate, gram positive, gelatin liquefying, nitrate reducing schizomycete were plated from the center of such nodules and with it the disease was reproduced on sugar beets in Washington. Peklo also reproduced it in Bohemia using transfers from cultures which I had sent to Král in Prague. The organism made Uschinsky's medium very viscid and in it enormously thick-walled capsules were formed. The bacterium did not grow in Cohn's solution, tolerated 9% NaCl in bouillon, resisted drying and measured 0.6 to 0.8 by 1.5 to 2 $\mu$ .

XIX. *Crown gall* (1892-3 and 1904-1922) due to *Bacterium tumefaciens* S. and T. My first acquaintance with this disease was on peach trees from California and the South in 1892-3, at which time an effort was made, chiefly by means of the microscope, to find in the galls a fungous or plasmodial parasite. As nothing constant of this nature was discovered and bacteria were not then in mind the subject was dropped. It was taken up again in 1904 on Paris daisies received from New Jersey with bacteria definitely in mind because the overgrowths superficially resembled olive tubercle in which I was then interested and knew from pure culture inoculations to be due to bacteria. The history of the early work is recorded in Bulletin 213 and need not be repeated here.

Earlier than any positive work in the United States Department of Agriculture, Cavara in Italy studied the disease as it occurs on grapes, isolated a white organism and with it produced a few tumors, but of this I knew nothing until I began gathering together the literature references for Bull. 213 after completion of our first researches.

The general outcome of these researches, continued for many years and still going on, has been an entire revision of views as

to the nature of the disease and as to sanitary measures necessary for its restriction. We now know not only the morphology and biology of the organism causing the tumors but also that the type of the tumor varies with the part infected, that there are several strains of the organism and probably many, that isolations differ in virulence, that some colonies which look all right have no virulence whatever, that on culture media and probably in the plant some strains lose virulence much sooner than others, that isolations are cross-inoculable to a very surprising degree, i. e., to plants of many families, that some plants, immune or nearly immune to certain strains, respond vigorously to other strains, and that some species are resistant to all strains so far as tested, e. g., olive, onion and garlic.

Things not yet determined are the number of strains, the extent of cross inoculability, the cause of resistance, the reason for loss of virulence on media, the nature of certain beet tumors and the question whether right looking but non-infectious colonies from such tumors and occasionally from other tumors are really the parasite deprived of infectious powers by sojourn in the plant under unfavorable conditions or are only deceiving saprophytes, extent of variability of the organism on culture media and in the tumor, production of metastasizing tumors in animals, etc.

The organism is a white, polar-flagellate, short rod, forming usually small, circular, smooth, watery, translucent colonies on agar poured plates. It grows on all the common culture media but must be transferred frequently. In media and in the tumors Y-shaped or other involution forms are common and often the colonies from tumors come up very slowly on poured plates. It does not stain by Gram and we have not been able to stain it in the tissue so as to distinguish it but it can be plated readily from young tumors. For further information, consult Bulletin 213 and the text-book where many interesting details are given.

The disease occurs on many kinds of plants in all parts of the world, most destructively on grapes, almonds, peaches and raspberries. Under favorable conditions the tumor begins to be visible within a week of making the needle pricks and continues to grow for several months or even for several years. One inocu-

lation does not protect from another. Unlike the preceding there are no bacterial cavities nor has the parasite been seen in the intercellular spaces or vessels. In the tumor tissue it occurs inside the cells and does not multiply so as to destroy them but only so as to stimulate them to divide repeatedly. Nellie A. Brown and Lucia McCulloch have been associated with me for many years in the study of this disease and the organism which causes it.

XX. *Soft rots*. The writer has also verified most of the statements of Prof. L. R. Jones on *Bacillus carotovorus*, of F. C. Harrison on *Bacillus solanisaprus*, of Otto Appel on *Bacillus phytophthorus*, of C. J. J. van Hall on *Bacillus atrosepticus*, and of Pethybridge and Murphy on *Bacillus melanogenes*, all of these isolations having passed through my hands. With exception of *B. atrosepticus* I found all of these organisms able to attack actively full grown parenchymatic tissues especially those full of water and to some extent also soft green shoots. They are not vascular diseases. Part of my conclusions have been published in the text-book and others await publication.

XXI. *Fire blight of pome fruits*. In the same way I have verified most of the statements of T. J. Burrill, J. C. Arthur, Merton B. Waite and others, respecting *Bacillus amylovorus* (Burrill) Trevisan, the cause of fire blight of apples, pears, quinces and other pome fruits. Trevisan never saw the disease but only changed the name of the organism. Exclusive of the soft rots this is probably our most destructive bacterial disease. It attacks leaves, shoots, flowers and young fruits and in general any rapidly growing meristematic tissues. Old tissues are not easily infected. Its most conspicuous effects are on shoots of the season but it is not confined to such but often attacks the cortex of large branches or of trunks and roots during the warmer part of the growing season, girdling and killing many trees. In late spring and early summer the tops of trees may be spotted all over with blackened shoots and dead leaves of flower clusters. The organism is transmitted by many leaf puncturing and nectar-sipping insects and enters through the uninjured nectaries or through wounds. It is carried over winter in restricted spots on exceptional trees, as first shown by Waite,

which spots ooze insect-frequented infectious fluids in the spring. During the summer also the organism comes to the surface easily and copiously through rifts and through natural openings (stomata) and is often distributed by the pruning knife. Many varieties of apples and pears are subject to the disease, some much more than others. The disease occurs all over the United States and has been introduced into Japan and New Zealand. I am in doubt as to its distribution in Europe. There are resistant species of *Pyrus* in Eastern Asia. A summary of my conclusions with many figures will be found in the text-book (pp. 359-388).

The above diseases fall into several intergrading types: (1) Vascular: *a*, xylem diseases, *b*, phloem diseases; (2) parenchymatic: *a*, diseases of immature parenchyma (blights, spots, cankers), *b*, soft rots, able to attack mature fruits and fleshy roots; (3) surface growths between closely appressed organs; (4) tumors of at least two different types: *a*, tubercles, *b*, crown-galls.

The organisms enter the plant in various ways: (1) through natural openings (nectaries, water-pores, stomata, lenticels), or (2) through wounds (broken roots, insect injuries, wind injuries, hail lesions, cuts due to pruning knives, etc.).

The reactions of the plant are changes in form and color, yellowing, reddening, greening, blackening, veining, spotting, twisting, loss of foliage, dwarfing, shrivelling, gum-exudate, cork-formation and in some cases overgrowths with or without excessive multiplication of shoots and roots. One attack of a disease so far as known does not protect from a second attack. Recovered plants may still harbor the parasite.

The schizomycetes causing these diseases are all non-sporiferous so far as known, and are quickly killed by exposure for a few minutes to temperatures considerably under that of boiling water (50° or 60° C. for 10 minutes). Very few of them grow at blood temperatures but many will grow (slowly) at or near 0° C. Most grow readily on a variety of culture media. The optimum growth temperature for most lies between 25° and 35° C. A few are rather sensitive to growth conditions (acids, alkalies, temperatures) and also must be transferred at frequent intervals. Some lose virulence readily on media; others

lose virulence for some species of plants before they do for others. Many are seed-borne or insect carried. Some are carried over winter in cankers.

I recognize three genera based on morphological characters: (1) *Bacillus* (peritrichiate flagellate); (2) *Bacterium* (polar flagellate); (3) *Aplanobacter* (non-motile); and separate the species chiefly on pathogenicity, reaction to stains, to culture media, to acids, alkalies, germicides, temperature (maximum, minimum and optimum for growth), dry air, etc.

The reasons I have given for my views on nomenclature of genera may be found in *Bacteria in Relation to Plant Diseases*, Vol. I, pp. 154-174, and still hold good. All descriptions in natural history should be based as far as possible on morphology. I have no sympathy with those who would make a genus for every species, nor, on the other hand, with those who would lump everything indiscriminately.\* Science is not advanced by either process. Nor do I think that chemical and physiological attributes are a satisfactory basis for distinguishing genera, e. g., the species I have mentioned ought not to be put into one genus (*Erwinia*) simply because they are plant parasites. (*Journal of Bacteriology*, 1917, p. 547.) Eventually I hope to give a key to all the species parasitic on plants but am not ready to do so now.

In recent years the study of crown gall has absorbed much of my time. As early as 1907 I conceived the idea from the involved microscopic structure of the tumor that its study might throw light on cancer and the more I have studied it the more analogies I have discovered. I have recorded the results of my studies and speculations in Bulletin 255 and in various medical and other journals and have recently summarized the whole subject with good figures in my text-book so that I shall touch on it here very briefly and only do so because any light that can be thrown from any branch of science upon the obscure subject of cancer ought to be welcome.

The crown gall tumor is an astonishing hyperplasia in which there are no bacterial cavities. It is a tumor not subject to physiological control and is of no use to the plant. The bacteria multiply within the rapidly dividing cells, which they do not kill,

but only mildly stimulate. We have not been able to demonstrate them in the tissues under the microscope with any certainty but they can be isolated by the methods of the bacteriologist. With virulent pure cultures and sensitive plants it is possible to obtain 100% of infections with none whatever in the controls. We have done this repeatedly on various plants. The organism causing this tumor is a medium sized, white, polar flagellate rod, able to grow in a variety of culture media. It is a wound parasite and no special carriers have been discovered. Usually the tumor begins to be visible a few days after the needle pricks are made, grows rapidly, if the plant is in good condition, dwarfs, crushes and destroys the surrounding parts and kills or injures the whole plant. Its action on the plant as a whole depends on its location, on the species susceptibility, on individual resistance, on the virulence of the strain, etc. Always in crown gall as in all other parasitic diseases two variable factors are to be kept in mind continually; (1) the virulence of the parasite, (2) the resistance of the host. He who keeps the parasite only in mind can have but a very one-sided and imperfect conception of the etiology of a disease. (The Journal of Cancer Research, Vol. V, pp. 243-260, July, 1920.) Generally the progress of crown gall, especially on trees and shrubs, is slow. The tumor may be largely parenchymatic in which case it dies early leaving an open wound with new tumor tissue developing on its margin, or hard and woody from the inclusion of many tracheids in which case it grows slowly and often persists for a long time. The tumor has a stroma consisting of supporting cells and vessels. It has no capsule and grows from its periphery converting surrounding cells of the same type (cortex cells) into tumor lobes. The primary tumor often develops tumor strands out of which grow secondary tumors having the structure of the primary tumor. Such strands occur in leaves and stems, in the region of the primary vessel parenchyma (near the pith) or in the bark cortex. The cells and vessels of the tumor, often under great pressure, are disoriented as much as in any cancer, and the nuclear parts are in excess taking a deep stain with haematoxylin and other animal tumor stains. The tumor may be cut out but if the excision is not complete it returns. It is the only

plant tumor which at all resembles cancer. As the ordinary tumor develops out of connective tissue (cortex) I have likened it to sarcoma. When the tumor grows in the vicinity of dormant buds or root-anlage they are stimulated into premature but structurally normal development, but when these totipotent or pluripotent cells are actually mingled with the tumor tissue, as often happens, then we have a development in the tumor of great numbers of abortive shoots and roots, so that I have likened this type of crown gall to an embryoma, meaning, of course, a solid malignant tumor in the sense of Wilms and Askanasy.

To the foregoing diseases might be added fragmentary observations on twice as many more, some of which have since been described by my assistants or are now being worked upon and all of which are genuine bacterial diseases. Among these might be mentioned *Pelargonium* leaf spot, *Delphinium* leaf spot, *Begonia* leaf spot, cauliflower leaf spot, rot of iris, rot of celery, soft rot of hyacinth, ash canker, several lettuce rots, wilt of beans, spot disease of soy beans, *Tropaeolum* wilt, *Tropaeolum* leaf spot, bud rot of canna, *gladiolus* disease, oat leaf spots, basal glume rot of wheat, bacterial disease of banana, bud rot of coconut, wild fire of tobacco.

Important researches are now in progress in my laboratory on germicides for seed-borne diseases, on the standardizing of beef infusion culture media, and on the colorimetric and potentiometric pH reactions of all the various organisms we are studying. The last paper published (by F. V. Rand) is on Pecan Rosette, one of the mosaic diseases.

Aside from botanical and bacteriological training the things that have most helped me have been: (1) persistence along a previously determined line of work (a matter of inheritance); (2) fondness for all forms of art and a desire for perfection (again an inheritance); (3) keen vision and ability to discriminate slight differences in form and color; (4) early familiarity with the technic of photography and photomicrography; (5) ability to assimilate quickly the literature of several modern languages. Handicaps have been (1) insufficient mathematical physics; (2) insufficient biochemistry. But perhaps if I had first specialized on these subjects, I should in the end have been

like one of my teachers who decided he would be a naturalist but must first get a good ground-work in Latin and Greek. The result was that he never passed on into a study of nature but became a teacher of the classics. Moral: There is not room in one short life for everything.

As I look back over my work in the U. S. Department of Agriculture I realize that I have been fortunate in being able to devote so large a part of my time to research; in having begun at the right time or in having discovered interesting subjects; in having had pleasant surroundings, congenial associates and faithful helpers; and finally in having had good laboratory and library facilities and great freedom of action. But after all I consider that my most important work has been the stimulating of other persons to undertake researches first in my own laboratory and then through my writings and teachings in many other laboratories.

Finally, after many years, I am again interested in *Peronosporaceae*, the blue-mold having appeared on tobacco in Florida.

# THE PUBLISHED WRITINGS of ERWIN FRINK SMITH

Compiled

by

FREDERICK V. RAND

## CONTENTS

Fungus diseases .....	47
Virus diseases .....	49
Bacterial diseases .....	50
Miscellaneous .....	50
Books .....	54
Crown gall (Plant cancer) .....	55
Smith-Fischer controversy .....	58
Miscellaneous scientific writings .....	58
Reviews .....	61
Biographies .....	67
Non-scientific writings .....	68

## Fungus Diseases

- 1885 Mich. Crop Report No. 50: 3-5, Dec. 1885.  
The potato rot. [Reprinted in part in Ann. Rept. for 1885, Agric. Exp. Station, Wisc. 3: 56-59. 1886.]
- 1886 U. S. Dept. Agric., Bot. Div. Bull. [2]. pp. 45-53, 1886.  
Appendix A. Synopsis of replies to a circular relative to grape mildews and grape rot in the United States. 7 pl. (3 col.) 4 fig. (In: F. Lamson-Scribner . . . Report on the fungous diseases of the grape vine, 136 p.)  
Mich. Farmer. March 9, 1886.  
The potato rot. A lecture delivered before the Washtenaw Pomological Society by Prof. V. M. Spalding of the University of Michigan. [Followed by extensive remarks by Erwin F. Smith.]  
A clipping.
- 1887 Rept. Comm. Agric. (1886): 133-134. Maps. 1887.  
Partial list of parasitic diseases of cultivated plants observed in Michigan by Erwin F. Smith. Maps. [Contains maps showing distribution of *Peronospora viticola* and of black rot of grapes in the United States, compiled from Department returns by Smith (following Rept. of Mycol. Sect.).]
- 1888 Botanical Gazette 13(8): 211-213. Aug. 1888.  
A date palm fungus (*Graphiola phoenicis* Poit).
- 1889 Journal of Mycology 5(1): 32-33. Mar. 1889.  
Spotting of peaches. [Observations on *Cladosporium carpophilum*.]  
—— 5(3): 123-134. Sept. 1889.

- Peach rot and peach blight (*Monilia fructigena*, Persoon). [Original observations by Smith.]  
 Journal of Mycology 5(4) : 203-204. Dec. 1889.  
 Prevalence of ergot in 1889. [Observations on *Claviceps purpurea*.]
- 1891 ——— 6(3) : 107-110. Jan. 6, 1891.  
 Field notes—1890. [Peach leaf curl. Plum *Taphrina*. Plum blight. Apple blight. Pear-leaf blight. Black rot. Vine blight. Brown rot of the peach. Peach yellows. The peach rosette. "Phenomena too fragmentary to be worked upon separately, and yet sometimes of much value to others if accurately observed and duly recorded."]
- 1892 ——— 7(1) : 36-39. 2 pl. Sept. 10, 1891.  
 Peach blight. *Monilia fructigena*, Persoon.  
 ——— 7(2) : 88-95. Mar. 10, 1892.  
 Field notes, 1891. [Peach leaf curl. Peach mildew. Black spot of peaches (*Cladosporium*). Frosty mildew. Peach rust. Peach rot (*Monilia*). Peach yellows. Clubbed branches (of peach). Stem and root tumors (of peach). Peach rosette. Plum blight. Pear diseases. Sycamore blight (*Gloeosporium*).]
- 1893 Rept. State Hort. Assoc. Penna. Yr. 1893 : 54-58. 10 pl. 1893.  
 Diseases of fruits and the use of fungicides. [In: Agriculture of Pennsylvania, 1893.]
- 1894 Jour. Mycol. 7(4) : 373-377. 1 pl. Aug. 15, 1894.  
 Field notes, 1892. [A new melon disease. Grape powdery mildew (*Uncinula spiralis*). Apple scab (*Fusicladium dendriticum*). Pear blight (*Bacillus amylovorus*). Gooseberry leaf blight (*Gloeosporium ribis*). Black spot of the peach (*Cladosporium carpophilum*). *Cercospora*? on peaches. Peach mildew (*Sphaerotheca pannosa*?). Peach curl (*Taphrina deformans*). Wilting of peaches on the tree. Stem and root tumors. Root rot of the peach.]  
 Proc. Amer. Assoc. Adv. Sci. 43 : 289-290. Aug. 1894 (1895).  
 The watermelon disease of the South. [Abstract of paper on *Fusarium nivium* (provisionally named) at 43d Annual meeting, Brooklyn, N. Y., Aug. 1894; pub. by Permanent Secretary, Mar. 1895.]
- 1895 Amer. Nat. 29(342) : 583-584. June 1895.  
 Perithecial stage of the apple-scab fungus.  
 ——— 44 : 190-191. Aug.-Sept. 1895 (1896).  
 The watermelon wilt and other wilt diseases due to *Fusarium*. [Abst. of paper at 44th Annual meeting, Springfield, Mass., Aug.-Sept. 1895; pub. by Permanent Secretary, May 1896; "this paper will be printed by the U. S. Dept. of Agric."]  
 ——— 44 : 191. Aug.-Sept. 1895 (1896).  
 The southern tomato blight. [Abstract of paper at 44th Annual meeting, Springfield, Mass., Aug.-Sept. 1895; pub. by Permanent

- Secretary, May 1896; cause, "a bacillus the biology of which has not been fully worked out;" "this paper will probably be printed as a part of a bulletin by the U. S. Dept. of Agric."]
- 1899 U. S. Dept. Agric., Div. Veg. Physiol. and Path. Bull. 17. 1899.  
 Wilt disease of cotton, watermelon, and cowpea (*Neocosmospora* nov. gen.). 72 p. 10 pl. (1 col., 1 double).  
 Sci. Amer. Suppl. 48(1246): 19981-19982. Nov. 18, 1899.  
 The fungous infestation of agricultural soils in the United States. [Paper read at Ohio State University, Aug. 25, 1899, before Bot. Sect., Amer. Assoc. Adv. Sci. meeting at Columbus, Ohio; "a full account of these diseases will be published in Bull. 17, Div. Veg. Physiol. and Path., U. S. Dept. Agric."]
- 1904 U. S. Dept. Agric., Bur. Plant Indust. Bull. 55. Feb. 16, 1904.  
 The dry rot of potatoes due to *Fusarium oxysporum*. By Erwin F. Smith and Deane B. Swingle. 64 pp. 8 pl. (including frontispiece), 2 fig.
- 1907 Science 26(663): 347-349. Sept. 13, 1907.  
 The parasitism of *Neocosmospora*—Inference versus fact.
- 1909 ——— 30(758): 60-61. July 9, 1909.  
 Diplodia disease of maize (suspected cause of pellegra). By Erwin F. Smith and Florence Hedges.
- 1910 ——— 31(802): 754-755. May 13, 1910.  
 A Cuban banana disease. [Abst. of paper before Amer. Phytopath. Soc., 1st Annual meeting, Dec. 30-31, 1919; reprinted in West. Ind. Comm. Circ. 25: 325-326. July 5, 1910, under general title: "Banana disease in America and Cuba;" also in: Hawaiian For. and Agric. 8: 33-35. 1911; "the fungus may be designated for the present as *Fusarium cubense*."]
- 1921 U. S. Dept. Agric. Dept. Cir. 174. Apr. [20], 1921.  
 A dangerous tobacco disease appears in the United States. By Erwin F. Smith and R. E. B. McKenney. 6 pp.  
 ——— 176. May 1921.  
 Suggestions to growers for treatment of tobacco blue-mold disease in the Georgia-Florida district. By Erwin F. Smith and R. E. B. McKenney. 4 pp.  
 ——— 181. June 7, 1921.  
 The present status of the blue-mold (*Peronospora*) disease in the Georgia-Florida district. By Erwin F. Smith and R. E. B. McKenney. 4 pp.

### Virus Diseases

- 1888 U. S. Dept. Agric., Bot. Div., Sect. Veg. Path. Bull. 9. 1888.  
 Peach yellows: a preliminary report. 254 p. 37 pl. (6 col.), 9 col. maps. [Exact date of issue not given but letter of submittal dated Nov. 10, 1888, and "1888" on cover.]

- 1889-91 Proc. Amer. Pomol. Soc. 22: 38-41. 1889; 23: 21-26. 1891.
- 1891 The chemistry of peach yellows. I, II. [Papers at 22nd session, Ocala, Fla., Feb. 20-22, 1889, and pub. by Society, 1889; and at 23rd session, Washington, D. C., Sept. 22-24, 1891, and pub. by Society, 1891.]
- 1890 Journal of Mycology 6(1): 15-16. May 14, 1890.  
What to do for peach yellows.
- 1891 Trans. Peninsula Hort. Soc. [Del.] 4: 55-61. 1891.  
Peach yellows. ["Synopsis of an address", 4th annual session, Easton, Md., Jan. 20-22, 1891.]  
Journal of Mycology 6(4): 143-148. 6 pl. Apr. 30, 1891.  
The peach rosette.  
U. S. Dept. Agric., Div. Veg. Path. Bull. 1. 1891.  
Additional evidence on the communicability of peach yellows and peach rosette. 65 p. 38 pl. 1891. [1. Peach yellows; 2. peach rosette. Exact date of issue not given.]
- 1892 Proc. Amer. Assoc. Adv. Sci. 41: 224-225. Aug. 1892.  
On the value of wood ashes in the treatment of peach yellows. [Abstract of paper at 41st meeting, Rochester, N. Y., Aug. 1892; pub. by Permanent Secretary, Dec. 1892. "This paper will be printed in substance in Dept. of Agric. Bulletin."]  
——— 41: 226. Aug. 1892.  
On the value of superphosphates and muriate of potash in the treatment of peach yellows. [Abstract of paper at 41st meeting, Aug. 1892; pub. by Permanent Secretary, Dec. 1892.]
- 1893 Journal of Mycology 7(3): 226-232. May 15, 1893.  
Additional notes on peach rosette.  
U. S. Dept. Agric., Div. Veg. Path. Bull. 4. 1893.  
Experiments with fertilizers for the prevention and cure of peach yellows, 1889-92. 197 p. 33 pl. (6 fold., 11 col.) [Exact date of issue not given.]  
Rept. State Hort. Assoc. Penna. Yr. 1893: 42-49.  
Peach yellows. 10 pl. [In: Agriculture of Pennsylvania, 1893.]
- 1894 U. S. Dept. Agric. Farmers' Bull. 17. May 1894.  
Peach yellows and peach rosette. 20 p. 7 fig.
- 1898 Fennville Herald, Oct. 15, 1898.  
Notes of the Michigan disease known as "little peach." [An address before the Saugatuck and Ganges Pomological Society, Fennville, Mich.; reprinted from Fennville Herald, Oct. 15, 1898. 12 p.]

## Bacterial Diseases

### Miscellaneous Bacterial Diseases

- 1894 Proc. Amer. Assoc. Adv. Sci. 42: 258-259. Aug. 1893 (1894).  
Two new and destructive diseases of cucurbits: (1) The muskmelon *Alternaria*; (2) A bacterial disease of cucumbers, cantaloupes

- and squashes. [Abst. of paper at 42d meeting, Madison, Wisc., Aug. 1893; published by Permanent Secretary, 1894.]
- 1895 Centralbl. Bakt. II. Abt. 1 (9/10) : 364-373. Apr. 30, 1895.  
*Bacillus tracheiphilus* sp. nov., die Ursache des Verwelkens verschiedener Cucurbitaceen.
- 1896 U. S. Dept. Agric., Div. Veg. Physiol. and Path. Bull. 12, Dec. 19, 1896.  
 A bacterial disease of the tomato, eggplant, and Irish potato (*Bacillus solanacearum* n. sp.). 28 p. 2 pl. (1 col.)
- 1897 Centralbl. Bakt. II. Abt. 3 (11/12) : 284-291. 2 pl. July 7; (15/16) : 408-415. Aug. 8; (17/18) : 478-486. 1 col. pl. Sept. 10, 1897.  
*Pseudomonas campestris* (Pammel). The cause of a brown rot in cruciferous plants.
- 1898 U. S. Dept. of Agric. Farmers' Bull. 68. Jan. 8, 1898.  
 The black rot of cabbage. 22 p.  
 Proc. Amer. Assoc. Adv. Sci. 46 : 274. Aug. 1897 (1898).  
 Wakker's hyacinth bacterium. [Abstract of paper at 46th meeting, Detroit, Mich., Aug. 1897; published by Permanent Secretary, June 1898.]  
 ——— 46 : 288. Aug. 1897 (1898).  
 On the nature of certain pigments produced by fungi and bacteria, with special reference to that produced by *Bacillus solanacearum*. [Abst. of paper at 46th meeting.]  
 ——— 46 : 288-290. Aug. 1897 (1898).  
 Description of *Bacillus phaseoli* n. sp., with some remarks on related species. [Paper at 46th meeting.]  
 Zeitschr. Pflanzenkr. 8(3) : 134-157. 1 pl. July 20, 1898.  
*Pseudomonas campestris* (Pammel) Erw. Smith: Die Ursachen der "Braun-" oder "Schwarz-" Trocken-Fäule des Kohls.  
 Trans. Peninsula Hort. Soc. [Del.] 11 : 142-147. 1898.  
 Some bacterial diseases of truck crops. [Paper at the 11th Annual session, Snow Hill, Worcester Co., Md., Jan. 12-14, 1898, on "Wilt of the cucumber, brown rot of the potato, and black rot of the cabbage"; published 1898, by "Press of the Delawarean."]   
 Proc. Amer. Assoc. Adv. Sci. 47 : 422-426. Aug. 1898.  
 Notes on Stewart's sweet-corn germ, *Pseudomonas stewarti* n. sp. [Paper at the 47th meeting and 50th anniversary of the Association, Boston, Mass., Aug. 1898; published by the Permanent Secretary, Dec. 1898.]
- 1901 U. S. Dept. Agric., Div. Veg. Physiol. and Path. Bull. 26. Feb. 21, 1901.  
 Wakker's hyacinth germ, *Pseudomonas hyacinthi* (Wakker). 45 p. 1 col. pl. 6 fig.  
 ——— 28. Aug. 6, 1901.  
 The cultural characters of *Pseudomonas hyacinthi*, *Ps. campestris*,

- Ps. phaseoli, and Ps. stewarti—four one-flagellate yellow bacteria parasitic on plants. 153 p. 1 fig.
- 1903 U. S. Dept. Agric. Bur. Plant Indust. Bull. 29. Jan. 17, 1903.  
The effect of black rot on turnips: a series of photomicrographs, accompanied by an explanatory text. 20 p. Frontispiece. 13 pl. Science 17(429): 456-457. Mar. 20, 1903.  
Observations on a hitherto unreported bacterial disease, the cause of which enters the plant through ordinary stomata. [On Pseudomonas pruni.]  
——— 17(429): 457. Mar. 20, 1903.  
Completed proof that P. stewarti is the cause of the sweet corn disease of Long Island. [On Pseudomonas stewarti.]
- 1904 ——— 19(480): 416-417. Mar. 11, 1904.  
The olive tubercle. By Erwin F. Smith and James B. Rorer.  
——— 19(480): 417-418. Mar. 11, 1904.  
The bacterial leaf spot disease. [“. . . to call attention to the fact that bacterial infection of plants through the ordinary stomata is not at all infrequent." Several cases mentioned, among them the larkspur leaf spot, for the organism of which the "name of Bacillus delphini is suggested."]  
Centralbl. Bakt. II. Abt. 15(22/23): 279-736. Dec. 10, 1904.  
Ursache der Cobb'schen Krankheit des Zuckerrohrs.
- 1905 Science 21(535): 481-483. Mar. 31, 1905.  
The effect of freezing on bacteria. By Erwin F. Smith and Deane B. Swingle. [Paper at 6th Annual meeting, Society of American Bacteriologists.]  
——— 21(535): 488-489. Mar. 31, 1905.  
Exhibition of cultures on starch jelly and on silicate jelly. [Abstract of paper at 6th Annual meeting, Soc. Amer. Bacteriologists.]  
——— 21(535): 500-502. Mar. 31, 1905.  
The bud rot of coconut palm in the West Indies. [Abstract of paper at 8th Annual meeting, Society for Plant Physiology and Morphology, Dec. 28-30, 1904; "The disease is the result of a bacterial rot of the terminal bud."]  
——— 21(535): 502. Mar. 31, 1905.  
Bacterial infection by way of the stomata in black spot of the plum. [Abst. of paper at 8th Annual meeting, Soc. Plant Physiol. and Morphol. Dec. 28-30, 1904.]  
——— 21(535): 502-503. Mar. 31, 1905.  
Burrill's bacterial disease of broom corn. By Erwin F. Smith and Florence Hedges. [Abst. of paper at 8th Annual meeting, Soc. Plant Physiol. and Morphol. Dec. 28-30, 1904.]  
Centralbl. Bakt. II. Abt. 15(7/8): 198-200. Sept. 23, 1905. 1 pl.  
Some observations on the biology of the olive-tubercle organism.
- 1906 Science 23(585): 424-425. Mar. 16, 1906.

- Channels of entrance and types of movement in bacterial diseases. [Abst. of address before the Society for Plant Physiol. and Morphol.]
- 1908 U. S. Dept. Agric., Bur. Plant Indust. Bull. 131. Part IV. May 13, 1908.  
Recent studies of the olive-tubercle organism. p. 25-43.  
—— 141. Part II. Aug. 31, 1908.  
The Granville tobacco wilt. p. 17-24.
- 1909 Science 30(763) : 223-224. Aug. 13, 1909.  
Seed corn as a means of disseminating *Bacterium stewarti*. [Abst. of paper before the Society of American Bacteriologists, Dec., 1908.]  
—— 30(763) : 224. Aug. 13, 1909.  
The occurrence of *Bacterium pruni* in peach foliage. [Abst. of paper before Soc. Amer. Bacteriol., Dec., 1908.]
- 1910 Science 31(802) : 748-749. May 13, 1910.  
Bacillus phytophthorus Appel. [Abst. of paper before the American Phytopathological Society, First Annual Meeting, Dec. 1909.]  
—— 31(803) : 792-794. May 20, 1910.  
Bacterial blight of mulberry. [Abst. of paper before the Amer. Phytopath. Soc., Dec. 1909; *Bacterium mori* (B. & L.), emend. E.F.S.]  
—— 31(803) : 794-796. May 20, 1910.  
A new tomato disease of economic importance. [Abst. of paper before Amer. Phytopath. Soc., Dec., 1909: preliminary description of *Bacterium* (?) *michiganense*.]
- 1912 Phytopathology 2(4) : 175. August 1912.  
Bacterial mulberry blight.  
—— 2(4) : 175-176. Aug. 1912.  
Bacillus coli, a cause of plant disease. [Smith here publishes a disclaimer from A. W. Giampietro that he reached independently the conclusion that onion rot is due to *B. coli*, as he was quite aware that John R. Johnston had discovered coconut bud-rot to be due to that organism.]  
—— 2(5) : 213-214. Oct. 1912.  
Isolation of pathogenic potato bacteria: A question of priority.
- 1914 —— 4(1) : 34. Feb. 1914.  
Identity of the American and French mulberry blight.
- 1915 Annals of the Missouri Botanical Garden 2(1/2) : 377-401. Feb.-Apr. 1915.  
A conspectus of bacterial diseases of plants.  
Journal of Agricultural Research 5(11) : 465-476. 7 pl. Dec. 13, 1915.  
Angular leaf-spot of cucumbers. By Erwin F. Smith and Mary Katherine Bryan.
- 1917 —— 10(1) : 51-54. 5 pl. July 2, 1917.  
A new disease of wheat. [Black chaff.]

- U. S. Dept. Agr., Bur. Plant Indust., Plant Disease Survey. Plant Disease Bulletin [1] (2) : 40. Sept. 1, 1917.
- Black chaff. [Statements by Smith respecting the bacterial disease of wheat described by him in the preceding entry.]
- 1918 Science 48(1228) : 42-43. July 12, 1918.
- Brown rot of Solanaceae on Ricinus. By Erwin F. Smith and G. H. Godfrey.
- U. S. Dept. Agr., Bur. Plant Indust., Plant Disease Survey. Plant Disease Bulletin 2(6) : 98-99. Map. July 15, 1918.
- Black chaff. [Note on the new bacterial disease of wheat, mostly on its distribution and on isolations.]
- 1919 Science 50(1280) : 48. July 11, 1919.
- The black chaff of wheat. [*Bacterium translucens* var. *undulosum* n. var. described.] By Erwin F. Smith, L. R. Jones, and C. S. Reddy.
- 50(1288) : 238. Sept. 5, 1919.
- Bacterium solanacearum* in beans. By Erwin F. Smith and Lucia McCulloch.
- 1921 Journal of Agricultural Research 21(4) : 255-262. 13 pl., map. May 16, 1921.
- Bacterial wilt of castor bean (*Ricinus communis* L.). [On *Bacterium solanacearum*.] By Erwin F. Smith and G. H. Godfrey.
- 1924 Phytopathology 14(1) : 48. Jan. 1924.
- A bacterial disease of broomcorn and sorghum. [Abst. of paper before Amer. Phytopath. Soc.] By Charlotte Elliott and Erwin F. Smith.
- 1925 Revue Gén. Sci. 36(5) : 134-139. Mar. 15, 1925.
- Les maladies bactériennes des plantes. [With résumé in: Revue Path. Vég. et Entomol. Agric. 12: 82-91. Jan.-Mar., 1925.]
- 1929 Journal of Agricultural Research 38(1) : 1-22. 9 pl. (2 col.). Jan. 1, 1929.
- A bacterial stripe disease of sorghum. [Extension of paper at 1924 meeting, Amer. Phytopath. Soc., supplemented by reference to an earlier manuscript by the late Erwin F. Smith, covering a review of the early literature and the investigations conducted by him and his assistants, 1904-1908; all color and photographic work by James F. Brewer.] By Charlotte Elliott and Erwin F. Smith.

#### Books

- 1905 Bacteria in relation to Plant Diseases. Vol. I, xii, 285 pp. 31 pl., 146 fig., Sept. 1905.
- Methods of work and general literature of bacteriology exclusive of plant diseases; bibliography pp. 203-266.
- 1911 ——— Vol. II, viii, 368 pp., 20 pl. (partly col.), 148 fig., Oct. 30, 1911. History, general considerations, and vascular diseases.

- 1914 Bacteria in relation to Plant Diseases. Vol. III, viii, 309 pp., 45 pl. (partly col.), 138 fig., August 4, 1914.  
Vascular diseases—continued. Carnegie Institution of Washington: Washington, D. C.
- 1920 An Introduction to Bacterial Diseases of Plants. xxx, 688 pp. Frontispiece (portrait of Burrill), 453 fig. 1920.  
[Literature references at end of most of the chapters.] W. B. Saunders Co.: Philadelphia and London, 1920.

Crown Gall (Plant Cancer)

- 1907 Science 25(643): 671-673. Apr. 26, 1907.  
A plant-tumor of bacterial origin. [*Bacterium tumefaciens* n. sp. described.] By Erwin F. Smith and C. O. Townsend.  
Centralbl. Bakt. II. Abt., 20(1/3): 89-91. Dec. 6, 1907.  
Ein Pflanzentumor bakteriellen Ursprungs. [On *Bacterium tumefaciens*.] By Erwin F. Smith and C. O. Townsend.
- 1909 Science 30(763): 223. Aug. 15, 1909.  
Etiology of plant tumors. [Abst. of paper before Society of American Bacteriologists, Dec., 1908.]
- 1911 Phytopathology 1(1): 7-11. 2 pl. Feb. 1911.  
Crown gall of plants.  
U. S. Dept. Agric., Bur. Plant Indust. Bull. 213. 215 pp. 36 pl. Feb. 28, 1911.  
. . . Crown gall of plants: its cause and remedy. By Erwin F. Smith, Nellie A. Brown, and C. O. Townsend.  
U. S. Dept. Agric., Bur. Plant Indust. Circ. 85. 4 pp. June 20, 1911.  
. . . Crown gall and sarcoma.
- 1912 Science 35(892): 161-172. Feb. 2, 1912.  
On some resemblances of crown-gall to human cancer. [Presidential address, Botanical Society of America, Washington, D. C., Dec. 28, 1911; reprinted as Publication No. 52, Bot. Soc. Amer., 1912.]  
Phytopathology 2(3): 127-128. June 1912.  
The staining of *Bacterium tumefaciens* in tissue.  
U. S. Dept. Agric., Bur. Plant Indust. Bull. 255. 61 pp. 109 pl., 2 diagr., June 29, 1912.  
. . . The structure and development of crown gall; a plant cancer. By Erwin F. Smith, Nellie A. Brown, and Lucia McCulloch.  
Centralbl. Bakt. II. Abt., 34(14/17): 394-406. July 20, 1912.  
Pflanzenkrebs versus Menschenkrebs. [Footnote: "Vortrag des abtretenden Präsidenten der Botanical Society of America, Washington, D. C., Dez. 28, 1911. Infolge einer Einladung waren auch Mitglieder der folgenden Vereine anwesend: Sect. G, A.A.A.S.; Soc. Amer. Bacteriologists; und Amer. Phytopath. Soc."]  
Phytopathology 2(6): 270-272. Dec. 1912.

- Etiology of crown galls on sugar beet. [Comments on Karl Spizer's article: "Über die Bildung des Zuckerrüben-Kröpfes."]
- 1913 Nat. Geogr. Mag. 24(1): 53-70. 12 pl. Jan. 1913.  
 [The Structure and development of crown gall.] The discovery of cancer in plants. An account of some remarkable experiments by the U. S. Department of Agriculture. With photographs by Dr. Erwin F. Smith. [Summarized from U. S. Dept. Agric., Bur. Plant Industry Bull. 255, with reproduction of many of its plates.]  
 Proc. 17th Internat. Congr. Med., London, pp. 281-298, Aug. 1913 (1914).  
 . . . Cancer in plants. [Sect. III, General Pathology and Pathological Anatomy. Part II, pub. 1914. Independent paper presented Monday afternoon, Aug. 11, 1913.]
- 1914 Compt. Rend. 1<sup>er</sup> Congr. Internat. Path. Comparée [Paris], (17-23 Oct., 1912) 2: 984-1002, 1914. [Preprint, Paris, 1912, 19 pp.]  
 Le cancer: Est-il une maladie du règne végétal?
- 1916 Science 43(1106): 348. Mar. 10, 1916.  
 Crown gall of plants and cancer. [Note on the closer tying up of crown gall to cancer.]  
 Jour. Cancer Research 1(2): 231-309. 25 pl. Apr. 1916.  
 Studies on the crown gall of plants: its relation to human cancer. [With bibliography of other papers bearing on the subject, by the author and his associates, p. 258; plates included in pagination.]  
 Jour. Agric. Research 6(4): 179-182. 6 pl. Apr. 24, 1916.  
 Crowngall studies showing changes in plant structures due to a changed stimulus. (Preliminary paper.)  
 Science 43(1121): 871-889. June 23, 1916.  
 Further evidence that crown gall of plants is cancer. [Paper read before Washington, D. C., Acad. Sci., May 11, 1916.]  
 Proc. Natl. Acad. Sci. 2: 444-448. Aug. 1916.  
 Further evidence as to the relation between crown gall and cancer. [Paper read before Academy, Apr. 18, 1916.]  
 Science 44(1139): 611-612. Oct. 27, 1916.  
 Tumors in plants. [First report of tumor production by products of bacterial growth only.]
- 1917 Jour. Agric. Research 8(5): 165-188. 62 pl. Jan. 29, 1917.  
 Mechanism of tumor growth in crown gall. [Literature cited, pp. 185-186.]  
 Proc. Natl. Acad. Sci. 3: 312-314. Apr. 1917.  
 Chemically induced crowngalls.  
 Proc. Amer. Philosoph. Soc. 56: 437-444. Aug. 3, 1917.  
 Mechanism of overgrowth in plants. [Paper read before the Society, Apr. 13, 1917.]  
 Bull. Johns Hopkins Hosp. 28(319): 277-294. 28 pl. Sept. 1917.  
 Embryomas in plants (produced by bacterial inoculations). [Ex-

- pansion of part of an address before Johns Hopkins Medical Society, Dec. 18, 1916.]
- 1918 Mem. Brooklyn Bot. Garden 1 : 448-453. July 1918.  
The relations of crown-gall to other overgrowths in plants. [Literature cited, p. 453.]
- 1919 Proc. Natl. Acad. Sci. 5 : 36-37. Feb. 1919.  
The cause of proliferation in *Begonia phyllomaniaca*. [Paper read before the Academy, Nov. 18, 1918.]
- 1920 Archives Dermatol. and Syphilol. 2(2) : 176-180. Aug. 1920.  
Production of tumors in the absence of parasites. [Varieties of tumors in plants—Experimental tumors in plants; paper read before Section on Dermatology and Syphilology, 71st Annual Session, American Medical Association, New Orleans, Apr. 1920.]
- 1921 Jour. Agric. Research 21(8) : 593-598. 10 pl. July 15, 1921.  
Effect of crown-gall inoculations on *Bryophyllum*.
- 1922 Jour. Cancer Research 7(1) : 1-105. 28 pl., 4 fig. Jan. 1922.  
Appositional growth in crown-gall tumors and in cancers. [Plates and legend pages in pagination; literature, pp. 47-49.]  
Phytopathology 12(6) : 265-270. 5 pl. June 1922.  
Fasciation and prolepsis due to crown-gall.
- 1923 Journal of Radiology 4(9) : 295-317. 8 pl. Sept. 1923.  
Twentieth century advances in cancer research (with special reference to etiology). [Paper at Annual Meeting, Radiological Society of North America, Detroit, Mich., Dec. 7, 1922.]
- 1924 Journal of Cancer Research 8(2) : 234-239. July 1924.  
Crown-gall and its analogy to cancer: A reply. [A reply to Levin and Levine's paper: "Malignancy of the crown-gall and its analogy to animal cancer."]  
Rev. Path. Vég. et Entomol. Agric. 11(4) : 219-228. Oct.-Dec. 1924.  
Le crown-gall.
- 1925 Jour. Heredity 16(2) : 60-62. Feb. 1925.  
The causes of cancer: A review [of B. J. Ellis Barker] (with Introduction by W. Arbuthnot Lane): "Cancer: How it is caused; how it can be prevented."]  
Science 61(1581) : 419-420. Apr. 17, 1925.  
Cancer in plants and in man. [Note by Smith on Ferdinand Blumenthal's work; note signed, "President of the American Association for Cancer Research, Berlin, March 5, 1925."]  
Science 61(1589) : 595-601. June 12, 1925.  
Some newer aspects of cancer research. [Opening address of the President, 18th Annual Meeting, Amer. Assoc. for Cancer Res., Navy Medical School, Washington, D. C., May 4, 1925.]  
Journal of Heredity 16(8) : 272. 1 pl. (frontispiece), Aug. 1925.  
[Plate, with descriptive text.]  
Tumor formation in *Bryophyllum*.

- 1926 Jour. Heredity 17(4): 112. 1 pl. (frontispiece), Apr. 1926.  
[Plate with descriptive text.]  
Fasciation of dahlia.  
Science 63(1637): 505. May 14, 1926.  
Changed structure due to a modified environment: A study of labile protoplasm in *Helianthus annuus* L. [Abst. of paper before National Academy of Sciences, Washington, D. C., Apr. 26-28, 1926; on crown-gall bacterial inoculations, and on proliferation induced by mechanical stimuli.]  
American Naturalist 60(668): 240-256. May-June 1926.  
Recent cancer research. [Footnote: "Address given at the Symposium on the Cancer Problem, American Society of Zoologists, Dec. 30, 1925."] Phytopathology 16(8): 491-508. 5 fig., Aug. 1926.  
A begonia immune to crown-gall: with observations on other immune or semi-immune plants. ["The first of a series of potentiometer studies."] By Erwin F. Smith and Agnes J. Quirk.  
1927 Mem. Natl. Acad. Science [Washington, D. C.] Vol. 22, 4th Memoir, 1 pl. 1., 50 pp. 43 pl., 1927.  
. . . Tumors, cysts, pith-bundles, and floral proliferations in *Helianthus*. [Description of plates, pp. 8-50; literature references, p. 5; paper presented at the Annual Meeting of the Academy, 1924.]

### The Smith-Fischer Controversy

- 1899 Centralbl. Bakt. II. Abt. 5(8): 271-278. Apr. 30, 1899.  
Are there bacterial diseases of plants? A consideration of some statements in Dr. Alfred Fischer's *Vorlesungen über Bakterien*. [With reply by Alfred Fischer. *Die Bakterienkrankheiten der Pflanzen*. Antwort an Herrn Dr. Erwin F. Smith, Assistant Pathologist, U. S. Dept. of Agriculture, Washington, D. C. 5(8): 279-287.]  
—— 5(23): 810-817. Dec. 1, 1899.  
Dr. Alfred Fischer in the role of pathologist.  
1901 —— 7(3): 88-100. Feb. 7; (4): 128-139. Feb. 23; (5/6): 190-199. 11 pl. Mar. 14, 1901.  
Entgegnung auf Alfred Fischer's "Antwort" in Betreff der Existenz von durch Bakterien verursachten Pflanzenkrankheiten. II.

### Miscellaneous Scientific Writings \*

\* For the years 1872-1886, Dr. Smith kept a scrap-book in which he pasted clippings of newspaper articles and poems written by him. This book is on file in the U. S. Dept. of Agric. Library, having been presented to the Library by Mrs. Erwin F. Smith.

- 1881 Catalogue of the phanerogamous and vascular cryptogamous plants of Michigan, indigenous, naturalized, and adventive. By Charles F. Wheeler and Erwin F. Smith. 105 p. Col. map. W. S. George & Co., State Printers and Binders, Lansing, 1881. [Also pub. in: Ann. Rept. Sec. State Hort. Soc. Mich. 10: 427-529. 1880 (1881), with caption, "Michigan flora, Prepared for the Michigan Horticultural Society by Charles F. Wheeler and Erwin F. Smith, Hubbardston, Michigan."]
- 1885 Ann. Rept. Mich. State Bd. Health, Suppl. (Sanitary Convention, Lansing), pp. 104-106.  
Sewerage and water-supply. Discussion at a Sanitary Convention held at Lansing, Michigan, March 19 and 20, 1885.  
——— Suppl. (Sanitary Convention, Ypsilanti), pp. 83-168. Lansing, 1885 (1886).  
The influence of sewerage and water-supply on the death rate in cities. [Read at the Sanitary Convention at Ypsilanti, Michigan, July 1, 1885.]  
Sanitary News 6: 179, Sept. 26, 1885.  
Does hygiene pay? By Max von Pettenkofer of Munich. Translated by Erwin F. Smith of Lansing, Mich.
- 1890 Entomol. Amer. 6(6): 101-103. June; (11): 201-208. Nov. 1890.  
The black peach aphid: a new species of the genus *Aphis*. [*Aphis persicaeniger* n. sp.]
- 1894-95 [Mycological and plant pathological definitions, in cooperation with Walter T. Swingle in "A Standard Dictionary of the English Language."] 2 v. and Supplement to the Subscription Edition. N. Y., Funk and Wagnalls, 1894-1897.  
NOTE: Dr. Smith in his "Synopsis of Researches of Erwin F. Smith in the U. S. Department of Agriculture, 1886-1922" states that the definitions beginning with D-G were written in cooperation with Walter T. Swingle. The list of the editorial staff accompanying the Supplement to the Standard Dictionary gives the letters C-H after the name of Walter T. Swingle.  
The botanical club check list: a protest. 16 pp. Privately printed, Washington, D. C., July 22, 1895.  
U. S. Dept. Agric., Farmers' Bull. 33. 1895. 24 pp. 21 fig. [Date of issue not given.]  
Peach growing for market.
- 1896 American Naturalist 30(353): 372-378. May; (354): 451-457. June; (355): 554-562. July 1896.  
The path of the water current in cucumber plants.  
Asa Gray Bulletin 4(3): 25-28. May; 4(4): 37-43. July 1896.  
Hints on the study of fungi. I, II. [A general paper on methods and apparatus, based on Smith's experience and primarily for "beginners."]

- U. S. Dept. Agric., Div. Veg. Physiol. and Path., Bull. 11. 45 pp. 1896. [Date of issue not given.]
- Legal enactments for the restriction of plant diseases. A compilation of the United States and Canadian laws.
- 1898 Trans. Mass. Hort. Soc. Yr. 1897. Part I: 117-133. 1898.
- The spread of plant diseases: A consideration of some of the ways in which parasitic organisms are disseminated.
- Proc. Amer. Assoc. Adv. Sci. 47: 411-412. Aug. 1898.
- Potato as a culture medium, with some notes on a synthesized substitute. [Abstract of paper at 47th meeting and 50th anniversary of Association, Boston, Mass., Aug. 1898; pub. by Permanent Secretary, Dec. 1898.]
- 47: 412-413. Aug. 1898.
- Some little-used culture media which have proved valuable for differentiation of species. [Abstract of paper.]
- 1899 Centralbl. Bakt. II. Abt., 5(3): 102-104. Feb. 21, 1899.
- Kartoffel als Kulturboden, mit einigen Bemerkungen über ein zusammengesetztes Ersatzmittel. ["Aus den Proceedings of the American Association for the Advancement of Science, v. 47, 1898."] American Naturalist 33(387): 208. Mar. 1899.
- Sensitiveness of certain parasites to the acid juices of the host plants. [Abstract of paper at 2d Annual meeting, Society for Plant Morphology and Physiology.]
- 33(387): 214-215. Mar. 1899.
- Gelatin culture media. [Abstract of paper at 2d Annual meeting, Society for Plant Morphology and Physiology.]
- 1902 Science 15(381): 601-612. Apr. 18, 1902.
- Plant pathology: a retrospect and prospect. [Presidential address, 5th Annual meeting, Society for Plant Morphology and Physiology, New York, Jan. 1, 1902.]
- 1907 3d International Conf. Genetics, Report (1906): 301-309. 1907.
- Abstract of an address on "Plant breeding in the United States Department of Agriculture." [Edited by Rev. W. Wilks, M.A., Secretary, Royal Horticultural Society; on hybridization (the cross-breeding of genera or species), the cross-breeding of varieties, and general plant-breeding.]
- Society of American Bacteriologists. Descriptive chart. F. D. Chester, F. P. Gorham, and Erwin F. Smith. (Committee on Methods of Identification of Bacterial Species.) [Endorsed by the Society for general use, at the Annual meeting, Dec. 31, 1907.]
- 1909 Science 30(763): 224. Aug. 13, 1909.
- Two sources of error in the determination of gas-production by microorganisms. [Abstract of paper at meeting of Soc. Amer. Bacteriol., Dec. 29-31, 1908.]
- 1911 U. S. Dept. Agric., Farmers' Bull. 478. 8 p. Dec. 21, 1911.

How to prevent typhoid fever. By Logan Waller Page, John R. Mohler, and Erwin F. Smith. [Introduction by James Wilson, Secretary of Agriculture.]

1923 Science 58(1507) : 396-397. Nov. 16, 1923.

Filing reprints. [Smith describes his method of filing.]

1929 Proc. Internat. Congr. Plant Sci. [4th], Ithaca, N. Y., Aug. 16-23, 1926, I: 13-46.

Fifty years of pathology. Portrait (Smith). 33 pl. (portraits). Geo. Banta, Menasha, Wisc. 1929. [Collection of portraits in U. S. Dept. Agric. Library.]

## Reviews

1882 School Moderator 2(34) : 219. [Grand Rapids, Mich.] May 11 1882.

[Paragraph on the bringing up of children, by Dr. Felix L. Oswalds, quoted from Popular Science Monthly, with comments by Smith. This appeared in a department of the School Moderator, called "Scientific and Sanitary," edited by Dr. Smith and signed "Tonia, Mich."]

——— 2(36) : 251. May 25, 1882.

[A diatribe against "nostrums" and "nostrum mongers" and their "scurrilous advertising." This appeared in a department of the School Moderator called "Scientific and Sanitary."]

——— 2(38) : 278-279. June 8, 1882.

[On a paper read before the State sanitary convention, Greenville, April 11, 1882, by E. P. Church, Supt., Greenville Schools. This appeared in a department of the School Moderator called "Scientific and Sanitary: School Hygiene."]

1884 ——— 5(15) : 269. Dec. 4, 1884.

The gardens about Haarlem. [Translated from Bull. Soc. Hort. Prat. Rhone.]

——— 5(1) : 6. Sept. 4, 1884.

Thoreau's note books. [Review of note books, 1840-1860. Thoreau "merits attention as a student and interpreter of nature. His books contain the aromatic, breezy spirit of the woods and meadows, and in spite of repetitions and narrow range, they are delightful reading . . . we are freshly reminded that this Concord hermit lived near to nature's heart, and held:—'The cunning-warded keys To all the woodcraft mysteries'."]

1885 ——— 5(20) : 384. Jan. 29, 1885.

Correspondence, Lansing, Mich., Jan. 22. [Letter on: "Text-books on physiology and hygiene approved for use in the schools of Michigan," with comments by Dr. Smith, who says "I have used Walker's book in the class-room for the last three months and am much pleased with it. . . ."]

——— 5(39) : 775. June 18, 1885.

- Physiology textbooks, approved by State Board of Health. [Report of action on two books approved, and on seven books apparently admissible for use in the schools; each book cited and briefly described.]
- Mich. Hort. 1(2) : 34-36. Oct. 1885.
- Recent literature concerning pear blight.
- 1889 Journal of Mycology. 5(1) : 44. Mar. 1889.
- Review of George Massee "On the presence of sexual organs in *Aecidium*."
- 5(1) : 46-48. Mar. 1889.
- Review of H. Marshall Ward, "A lily disease."
- 5(2) : 98-102. June 1889.
- Review of Oscar Brefeld, "Untersuchungen aus dem Gesamtgebiete der Mykologia."
- 5(2) : 107-111. June 1889.
- Review of Felix von Thümen, "Die Bekämpfung der Pilzkrankheiten unserer Culturgewächse."
- 5(3) : 174-175. Sept. 1889.
- Review of Dr. Robert Hartig, "Lehrbuch der Baumkrankheiten."
- 5(4) : 222-223. 1889. [Dec. ?]
- Review of Felix von Thümen, "Die Pilze des Aprikosenbaumes (*Armeniaca vulgaris* Lam.)."
- 1890-91 ——— 6(1) : 1-8. May 14; (2) : 59-71, Sept. 10, 1890; (4) : 153-164. Apr. 30, 1891.
- Recent investigations of smut fungi and smut diseases. [An address before the Society of Agriculturists of Berlin, February 17, 1888, by Oskar Brefeld; translation from "Nachrichten aus dem Klub der Landwirte zu Berlin," Nos. 220-222, by Smith.]
- 1891 ——— 6(3) : 117-118. Jan. 6, 1891.
- Review of W. A. Kellerman and W. T. Swingle, "Preliminary experiments with fungicides for stinking smut of wheat."
- 1892 ——— 7(2) : 135-140. Mar. 10, 1892.
- Review of Dr. Alfred Fischer, "Phycomycetes."
- 7(2) : 140. Mar. 10, 1892.
- Fruit culture in foreign countries. [Review of reports from Consuls of the United States on fruit culture . . . Washington, Govt. Printing Office, 1890, pp. 391-397.]
- 7(2) : 140-147. Mar. 10, 1892.
- Reviews of Louis Mangin, "Sur la callose, nouvelle substance fondamentale existant dans le membrane;" "Sur les reactifs colorante des substances fondamentale de la membrane;" and "Sur la désarticulation des conidies chez les Péronosporées."
- 7(2) : 148-149. Mar. 10, 1892.
- Review of H. Graf zu Solms-Laubach, "Fossil botany, being an introduction to palaeobotany from the standpoint of the botanist."

English translation by Garnsey. Revised by Balfour. Oxford, Clarendon Press, 401 pp. 1891.

- 1893 *Journal of Mycology* 7(3): 282-287. May 15, 1893.

Reviews of Dr. Wilhelm Zopf, "Die Pilze in morphologischer, physiologischer, biologischer und systematischer Beziehung"; and Dr. Oskar Kirchner, "Die Krankheiten und Beschädigungen unserer landwirtschaftlichen Kulturpflanzen."

- 1894 *Science* 23(575): 80-81. Feb. 9; (578): 115-118. Mar. 2, 1894.

Memorabilia botanica.—I-II. [I. Cabinet woods of the fair,—The uncertainties of common names,—The coffee disease of Ceylon,—Botany in secondary schools,—Embryophyta zoidiogama,—South Kensington. II. A journey into the tropics,—Parasitic algae,—Vegetable ferments,—Maples,—The fossil flora of S. E. France.]

*Journal of Mycology* 7(4): 381-382. Aug. 15, 1894.

Review of G. Haberlandt, "Eine botanische Tropenreise: Indo-Malayische Vegetationsbilder und Reiseskizzen."

— 7(4): 382-385. Aug. 15, 1894.

Review of Adolf Mayer, "Ueber die Mosaikkrankheit des Tabaks."

— 7(4): 389-396. Aug. 15, 1894.

Review of E. van Tavel, "Vergleichende Morphologie der Pilze."

— 7(4): 396-398. Aug. 15, 1894.

Review of H. Marshall Ward, "The diseases of conifers."

- 1895 *Science*, n. ser. 1(3): 77-78. Jan. 18, 1895. Length of vessels in plants.

[Notes culled from Strasburger's "Bau und Verrichtungen der Leitungsbahnen in den Pflanzen."]

*American Naturalist* 29(342): 583. June 1895. What becomes of the flagella?

[Review of Rothert, "Ueber das Schicksal der Cilien bei den Zoosporen der Phycomyceten."]

— 29(342): 584-585. June 1895.

Poisonous Cactaceae. [Review of Lewin, on the isolation of the alkaloid, anhalonin, from *Anhalonium lewinii*.]

— 29(342): 585. June 1895.

Rothert on heliotropism.

— 29(342): 585-586. June 1895.

Austro-German views on botanical nomenclature.

— 29(342): 586. June 1895.

Separation of enzymes. [Review of Thomas B. Osborne, "The proteids of the rye kernel;" "The proteids of barley;" and "The chemical nature of diastase."]

— 29(343): 615-621. July 1895.

The symbiosis of stock and graft. [Review of Hermann Vöchtling, "Ueber Transplantation am Pflanzenkörper."]

- American Naturalist 29(343): 671-674. July 1895.  
 The action of light on bacteria. [Review of H. Marshall Ward paper of above title in: Philosoph. Trans. Roy. Soc. London 185: 961-986. 1894.]  
 ——— 29(343): 674. July 1895.  
 The role of calcium and magnesium. [Review of Bokorny in: Bot. Centralb. 62:1.]  
 ——— 29(344): 749-750. Aug. 1895.  
 Woronin on Sclerotinia.  
 ——— 29(344): 750-752. Aug. 1895.  
 Demonstration of photosyntax by bacteria. [Review of Th. W. Engelmann.]  
 ——— 29(344): 752-753. Aug. 1895.  
 Detection of glukase by auxanographic methods. [Review of Beijerinck.]  
 ——— 29(345): 847-851. Sept. 1895.  
 Fischer on bacteria.  
 ——— 29(345): 851-854. Sept. 1895.  
 The mushroom gardens of South American ants.  
 ——— 29(346): 898-903. Oct. 1895.  
 Root tubercles of Leguminosae. [Review of Gonnermann in: Landw. Jahrb. 23(4/5): 649-671. 1894]  
 ——— 29(346): 933-936. Oct. 1895.  
 Bactericidal action of metals. [Review of Meade Bolton.]  
 ——— 29(347): 1009-1010. Nov. 1895.  
 Saccardo's color scale.  
 ——— 29(347): 1010-1011. Nov. 1895.  
 Kroeber's transpiration experiments.  
 ——— 29(348): 1100-1103. Dec. 1895.  
 [J.M.] Macfarlane on paraheliotropism.  
 ——— 29(348): 1103-1104. Dec. 1895.  
 Chalazogamy in Juglans regia. [Review of Dr. Treub, and Dr. Nawaschin papers in large part, and particularly: Bot. Centralbl. 63: 353-357. 1895.]  
 1896 ——— 30(349): 61-63. Jan. 1896.  
 Changes due to an alpine climate. [Review of Gaston Bonnier.]  
 ——— 30(349): 63-64. Jan. 1896.  
 Spore formation controlled by external conditions. [Review of Johann Bachmann.]  
 ——— 30(349): 64-65. Jan. 1896.  
 Germination of refractory spores. [Review of Jacob Eriksson.]  
 ——— 30(349): 65. Jan. 1896.  
 Botany at the British Association.  
 ——— 30(349): 65-66. Jan. 1896.  
 Nitrifying organisms. [Review of paper by Burri and Stutzer, presented at Bonn.]

American Naturalist 30(349): 66-67. Jan. 1896.

Relation of sugars to the growth of bacteria. [Review of Theobald Smith.]

——— 30(349): 67. Jan. 1896.

Algal parasite on coffee. [Review of F. A. F. C. Went, on *Cephaleurus coffeae* on coffee.]

——— 30(350): 120-122. Feb. 1896.

Recent books on vegetable pathology.

——— 30(350): 137-142. Feb. 1896.

Smut fungi by Oscar Brefeld. [Review.]

——— 30(351): 224. Mar. 1896.

Water pores. [Review of Anton Nestler.]

——— 30(351): 224-226. Mar. 1896.

Biology of smut fungi. [Review of Brefeld, 3d part, Heft XII of the "Untersuchungen."]

——— 30(351): 226-228. Mar. 1896.

Function of anthocyan. [Review of Leopold Kny.]

——— 30(352): 318-319. Apr. 1896.

Ambrosia. [Review of several workers on the subject: Schmittberger, Henry G. Hubbard, R. Goethe.]

——— 30(352): 319-321. Apr. 1896.

White ants as cultivators of fungi. [Review of several workers.]

——— 30(352): 321. Apr. 1896.

Desert vegetation. [Review of George Henslow's book "The origin of plant structures."]

——— 30(352): 321-323. Apr. 1896.

A second Rafinesque. [A review of "Die Pestkrankheiten . . . Kulturgewächse . . . geschildert von Prof. Dr. Ernst Hallier. Pub. in Stuttgart, 1895, by Erwin Nägler. 144 pp.]

——— 30(353): 405-408. May 1896.

Change in structure of plants due to feeble light. [Review of Gaston Bonnier.]

——— 30(353): 408. May 1896.

A graft hybrid. [Review of N. Wille.]

——— 30(353): 408-409. May 1896.

Ustilaginoidea. [Review of Brefeld.]

——— 30(354): 490-493. June 1896.

A new classification of bacteria. [Review of W. Migula in: Die Natürlichen Pflanzenfamilien, Lief. 129, 1896.]

1896-97 ——— 30(356): 626-643, Aug.; (357): 716-731, Sept.; (358): 796-804, Oct.; (359): 912-924, Nov. 1896; 31(361): 34-41, Jan.; (362): 123-138, Feb. 1897.

Bacterial diseases of plants: A critical review of the present state of our knowledge. I-VI.

1897 ——— 31(363): 228-231. Mar. 1897.

- What is *Leuconostoc mesenteroides*? [Review of several papers, particularly Liesenberg, and Zopf.]  
 American Naturalist 31(364): 312-314. Apr. 1897.
- Review of K. B. Lehmann and R. Neumann: "Atlas und Grundriss der Bakteriologie und Lehrbuch der specialen bakteriologischen Diagnostik." Teil I, Atlas, Teil II, Text. [Pub. by J. F. Lehmann, Munich, 1896.]  
 ——— 31(368): 717-720. 3 fig. Aug. 1897.
- Chemotropism of fungi. [Review of Manabu Miyoshi.]  
 1898 ——— 32(374): 96-110. Feb. 1898.
- [Report of] the first annual meeting of the Society for Plant Morphology and Physiology.  
 ——— 32(375): 208-210. Mar. 1898.
- Ripening fleshy fruits. [Review of C. Gerber.]  
 ——— 32(375): 210. Mar. 1898.
- Ferns of Nicaragua. [Review of B. Shimek.]  
 ——— 32(376): 284-287. Apr. 1898.
- Lessons with plants. [Review of L. H. Bailey.]  
 ——— 32(377): 365-369. May 1898.
- Zinsser on root tubercles of Leguminosae. [Review.]  
 ——— 32(378): 456-457. June 1898.
- Sulphur bacteria. [Review of Miyoshi.]  
 ——— 32(378): 457-459. June 1898.
- Ripening of cheese. [Review of Olav Johan-Olsen.]  
 ——— 32(380): 600. Aug. 1898.
- Recent inexpensive popular literature on mushrooms.  
 ——— 32(380): 601. Aug. 1898.
- Merrill on Lower California. [Review of George P. Merrill, on the ecology of the region.]  
 ——— 32(380): 602-603. Aug. 1898.
- Whitney on Florida. [Review of Milton Whitney on the soils, and particularly on the plant associations of the region.]  
 Science 8(202): 651-660. Nov. 11; (203): 690-700. Nov. 18, 1898.
- Botany at the anniversary meeting of the American Association for the Advancement of Science.—I, II. [Smith's report of Section G, with abstracts of papers given at meeting, apparently prepared by him from original papers or authors' abstracts; 50th anniversary and 47th meeting, Boston, Mass., Aug., 1898; Sect. G. organized Monday noon, Aug. 22nd (p. 651).]  
 1899 American Naturalist 33(385): 80. Jan. 1899.
- Moore's bacteriology. [Review of Veranus A. Moore, "Laboratory directions . . ."]  
 ——— 33(386): 169-170. Feb. 1899.
- Are bacteria fungi? [Review of Ol. Johan-Olsen.]  
 ——— 33(386): 170. Feb. 1899.

Dr. Bolander. [Review of paper by Willis L. Jepson on Dr. Henry N. Bolander, botanical explorer.]

American Naturalist 33(387): 199-217. Mar. 1899.

[Report on] the second annual meeting of the Society for Plant Morphology and Physiology. [Includes abstracts of papers given.]

——— 33(390): 540-543. June 1899.

A new work on lichens. [Review of Funfstück's account of Lichenes, 1st part, in: Die Natürlichen Pflanzenfamilien, Lief. 180.]

1910 Science 32(810): 56-58. June 8, 1910.

Review of Benjamin Minge Duggar: "Fungous diseases of plants. With chapters on physiology, culture methods and technique."

1912 Phytopathology 2(5): 214-215. Oct. 1912.

A new method in bacterial research. [Review of Churchman (Jour. Exp. Med., Aug. 1912), on a new test corresponding to and supplementing Gram's stain; with preliminary results of the test on 14 bacterial species, by Smith.]

1926 Science 63(1629): 305-307. Mar. 19, 1926.

Black chaff of wheat in Russia. [Review, particularly of Janczewsky in: Bull. Appl. Bot. and Plant Breed. 14(1): 377-385, with comments on Smith's own previous work.]

Journal of Heredity 17(7): 255-256. July 1926.

The story of the microbe hunters: A review. [Review of Paul de Kruif's "The Microbe Hunters."]

## Biographies

1899 Asa Gray Bulletin 7(1): 1-5. Feb. 1899.

Gilbert H. Hicks. Portrait (frontispiece).

1911 Phytopathology 1(1): 1-2. Feb. 1911.

Anton de Bary. Portrait (frontispiece).

1912 ——— 2(1): 1-4. Feb. 1912.

Woronin. Portrait (frontispiece).

1916 Journal of Bacteriology 1(3): 269-271. May 1916.

In memorium: Thomas J. Burrill. Portrait (plate).

1918 Science 48(1240): 335-336. Oct. 4, 1918.

Frank N. Meyer.

1920 Pasteur: The history of a mind. By Émile Duclaux . . . Translated by Erwin F. Smith and Florence Hedges. xxxii, 363 p. Frontispiece (portrait of Pasteur), 14 other portraits, 22 figs. W. B. Saunders Co., Philadelphia and London, 1920.

1923 Scientific Monthly 16(3): 269-279. Mar. 1923.

Pasteur: The man (Dec. 27, 1822-Sept. 28, 1895).

1925 ——— 21(4): 364-389. Oct. 1925.

Translation of Émile Roux: "The Medical Work of Pasteur." 2 portraits. [Footnote: "A generation has passed since this paper was written (1896) but it is still as interesting as ever. It is, in

fact, the best brief paper we have on the work of Pasteur, and the writer of this note believes many will like to have it in an English dress.—E.F.S.”]

1926 *Journal of Heredity* 17(10) : 366-367. Oct. 1926.

William Edwin Safford, the man. [Read before the Botanical Society of Washington, D. C., Feb. 2, 1926.]

*Nature Magazine* 8(6) : 346-347. Dec. 1926.

Louis Pasteur. Portrait.

### Non-Scientific Writings

1915 *For Her Friends and Mine: A book of aspirations, dreams and memories.* Printed privately, Washington, D. C., 381 pp., 13 text cuts by Erwin F. Smith, 1915. [With portrait of “Charlotte May Buffett, some-time wife of Erwin F. Smith,” with her biography; 12 original odes and songs and 197 sonnets by Erwin F. Smith; and 58 translations of poems from the German, French and Italian.]

1924 *Jour. Washington [D.C.] Acad. Science* 14(11) : 231-238. 1924.

Some thoughts on old age. [Address, May 4, 1924, at Annual Dinner, Botanical Society of Washington, as Guest of Honor, ending with original poem.]

SKETCHES ON THE LIFE AND WORK

of

ERWIN FRINK SMITH

Compiled

by

Frederick V. Rand

Brandes, Elmer W.

Erwin F. Smith. *Science* 66 (1713) : 383-385. Oct. 28, 1927.

Cañizo, José del.

Necrologia: Erwin F. Smith. *Bol. Patol. Vég. y Entomol. Agric.* 2(5/7) : 90-92. Portrait. Jan.-Sept. 1927.

Clinton, G. P.

Erwin Frink Smith (1854-1927). *Proc. Amer. Acad. Arts and Sci.* 70(10) : 575-578. 1936.

Ferraris, T[eodoro].

Erwin F. Smith (1854-1927). *Curiamo le Piante* 4(12) : 224-225. Portrait. Dec. 27, 1927.

[Hedges, Florence].

Dr. Erwin F. Smith, scientist, is dead. *U. S. Dept. Agric., Official Record* 6(16) : 1, 5, 8. Apr. 20, 1927.

Jones, Lewis Ralph, and Rand, Frederick V.

Erwin F. Smith, 1854-1927. *Jour. Bact.* 15(1) : 1-6. Portrait. Jan. 1928.

Jones, Lewis Ralph, Welch, Wm. H., and Rand, Frederick V.

To Erwin Frink Smith. *Phytopath.* 18(1) : 1-5. Jan. 1928. [Reproduction of testimonials, engrossed in brochure presented to Smith at time of dinner given in his honor at Philadelphia, December, 1926.]

Komuro, Hideo.

[On the late Erwin F. Smith, the discoverer of tumefaciens tumor.] *Kagaku [Science]* 2(7) : 293-296. Portrait. 1932. [In Japanese; with partial bibliography.]

L., C. L.

Erwin F. Smith. *Flor. Exchange* 64(16) : 1519. Apr. 16, 1927.

Magrou, Joseph.

Erwin F. Smith. *Rev. Path. Vég. et Entomol. Agric.* 14(2) : 160-163. Apr.-June 1927.

Makato, Hansewa.

[Erwin F. Smith, American phytopathologist, dies.] *Trans. Sapporo Nat. Hist. Soc.* 9(2) : 275-284. Portrait. 1927. [Text in Japanese; bibliography, pp. 279-284.]

Marchionatto, Juan B.

Erwin F. Smith en el décimo aniversario de su muerte. *Univ. Nacion. La Plata Pub. Ofic.* 21(7) : 47-56. Portrait. 1938.

Nakata, K.

[Reminiscences of Dr. Erwin F. Smith.] *Byôchû-gai Zasshi (Journal of Plant Protection)* 14(7) : 371-374. Portrait. July 1927. [In Japanese.]

Peattie, Donald Culross.

Erwin F. Smith—A young man's impression. *Scientific Monthly* 25 (July) : 84-86. Portrait (p. 88). 1927.

Rand, Frederick V.

Erwin F. Smith. *Mycologia* 20(4) : 181-186. Portrait. July-Aug. 1928.

Riker, E. J.

Erwin F. Smith. *Rev. Path. Vég. et Entomol. Agric.* 14(3) : 189-190. July-Sept. 1927.

Rosen, H. R.

Erwin F. Smith—Friend of youth. *Mycologia* 19(5) : 292-293. Sept.-Oct. 1927.

True, Rodney H.

Erwin F. Smith, 1854-1927. *Phytopath.* 17(10) : 675-688. Portrait. Oct. 1927 [Bibliography, pp. 680-688.]

Anonymous.

Erwin [F.] Smith honored by phytopathologists. Plant scientist, rounding out forty years in Department, eulogized at Society's dinner. *U. S. Dept. Agric., Official Record* 6(2) : 3. Jan. 12, 1927.

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Dinner in honor of Dr. Erwin F. Smith. *Science* 65(1676) : 154. Feb. 11, 1927.

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Doctor Erwin F. Smith. *Jour. Washington [D. C.] Acad. Sci.* 17(14) : 384. Aug. 19, 1927. [Obituary notice.]

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In memoriam: Erwin Frink Smith. 1854-1927. *Phytopath.* 18(5) : 475. May 1928.

### Degrees, Honors, and Society Memberships

University of Michigan, B.S. in Biology (1886), Sc. D. (1889), LL.D. (1922); University of Wisconsin, Sc. D. (1914).

Associate Editor, *Centralblatt für Bakteriologie* (II. Abt., first 25 volumes); Trustee, Marine Biological Laboratory, Woods Hole, Massachusetts (three terms); Certificate of Honor, American Medical Association, 1913, for cancer research in plants.

Member of: American Academy of Arts and Sciences (Fellow), American Association for the Advancement of Science (Fellow; President Section G, 1906), American Association for Cancer Research (Vice President, 1924; President, 1925), American Association for Medical Progress

ERWIN FRINK SMITH—JONES

(Life Member), American Philosophical Society, American Phytopathological Society (President, 1916), American Pomological Society (Life Member), Botanical Society of America (President, 1910), Council for National Defense, International Conference (Third) on Genetics, London, 1906, International Congress (First) of Comparative Pathology, Paris, 1912, International Congress (Seventeenth) of Medicine, London, 1913, National Academy of Sciences (Chairman, Botanical Section three years), National Association for the Study and Prevention of Tuberculosis, Society of American Bacteriologists (President, 1906), Society for Plant Morphology and Physiology (President, 1902), German Central Commission for Cancer Research and Control (Foreign Associate), Russian Botanical Society (Honorary Member, Mycological Section), Arts Club (Washington, D. C.) (member Executive Committee for Erection of a National Peace Carillon in Washington), Cosmos Club (Washington, D. C.), The Literary Society of Washington, D. C., National Arts Club (New York), National Carillon Association (an incorporator), and Phi Delta Theta Fraternity.



*Edwin H. Hall*

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BIOGRAPHICAL MEMOIR

OF

EDWIN HERBERT HALL

1855-1938

BY

P. W. BRIDGMAN

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## EDWIN HERBERT HALL

1855-1938

BY P. W. BRIDGMAN

Edwin Herbert Hall was born in North Gorham, then Great Falls, Maine, on November 7, 1855, the son of Joshua Emery Hall and Lucy Ann Hilborn Hall. On his father's side he was descended from John Hall, who came to this country from England early in the 17th century and settled at Dover, New Hampshire; from Anthony Emery who landed in Boston from England in 1635, the son of Jean Emery, a Frenchman who settled in England after the massacre of St. Bartholomew; and from Kenelson Winslow, who emigrated to this country probably in 1629 and settled in Marshfield, Massachusetts. He was of the ninth generation of each of these three lines. On his mother's side he was descended from Robert Hilborn, who was of Quaker stock and came to Maine probably from New Jersey about 1775. In spite of Robert Hilborn's Quaker stock, he enlisted twice for service in the American Revolution from Portland, Maine. Also on his mother's side his descent was probably from Nicholas Noyes, who came to Newburyport about 1635. For two generations back his ancestors were Maine country folk, engaged in the conventional occupations of the countryside. His father's father, Levi Hall, was born in Windham, Maine, in 1787, and moved to Great Falls. He was a farmer and probably also a stone mason, served as deputy sheriff with zeal, was an abolitionist, a "Washingtonian" teetotaler, and an officer in the war of 1812. His father's mother, Jane Emery, was born in Windham, Maine, in 1795. His mother's father, Samuel Hilborn, lived most of his life in West Minot, Maine, occupied as a farmer. His mother's mother was Nancy Noyes. His father, Joshua Emery Hall, was born in 1823 in Great Falls, now North Gorham, Maine, received a common school education with a little "academy" instruction, and was occupied with farming and lumbering. He served as selectman, representative in the Maine legislature, and justice of the peace. He died at the early age of 41, of some abdominal trouble, perhaps peritonitis. At the time of his death his old school teacher said of him in

the local paper, "I never *knew* a better scholar. His grade and grasp of mind were remarkable; his attainments, for his advantages, rare; his qualifications for usefulness, uncommon." His mother, Lucy Ann Hilborn, was born in 1825 in West Minot, Maine, and lived in West Minot and North Gorham until she came to Cambridge to live soon after her son's marriage. In youth she taught in a district school and worked in a textile factory. She was very well-read, had discriminating literary taste and a talent for drawing. She died at the age of 86. A brother of his mother, Samuel Greeley Hilborn, was Congressman from California.

Besides Edwin Herbert his father and mother had two other sons and two daughters. The daughters and one of the sons died in infancy or childhood. The surviving son, Frederic Winslow Hall, was born in 1860 and moved to California at the age of 20, where he is still living (February 1939), having retired from his practice of law.

Edwin Herbert describes himself as not living on a farm after 1867, when he was 12 years old, but continuing to work often as a farm hand during his boyhood and youth. He attended the usual district school, and prepared for college in two years at "Gorham Seminary". He entered Bowdoin in the fall before he was 16, and graduated at the head of his class four years later in 1875 with the A.B. degree. During college he taught two terms in the same district school which he had attended as a boy. He described himself as "an industrious, rather than a brilliant scholar, doing all my work fairly well, if not very easily.—The only real talent that I showed in college, if I showed any, was for writing. I was, perhaps, the most prolific editor of the college paper during my editorial year."

He did not decide on his life work immediately after graduation, but served during 1875-76 as principal of Gould's Academy, Bethel, Maine, and during 1876-77 as principal of the high school in Brunswick, Maine. It was not until after these two years of teaching that he decided to enter physics. He writes: "I should perhaps have studied law, if I had not felt myself unfitted to advocate a cause in which I did not believe. I turned to science, after two years of school teaching, because it was

progressive and satisfied my standards of intellectual and moral integrity, not because I had any passionate love of it or felt myself especially gifted for scientific undertakings." In the fall of 1877 he entered the Johns Hopkins University as a graduate student in physics, working under Rowland. Before entering the Johns Hopkins he explored the possibilities of study at Harvard, but was advised with engaging candor by John Trowbridge, the Director of the Harvard Physics Laboratory, to go to the Johns Hopkins because Harvard did not possess adequate facilities. In 1879 he discovered the "Hall" effect, which will presently be discussed in greater detail. He received the Ph.D. degree in 1880, and continued at the Johns Hopkins during 1880-81 as assistant in physics. In December 1880 he was appointed to a Tyndall Scholarship by the Johns Hopkins for study and travel abroad during the following year, but he later resigned this scholarship because in the spring of 1881 he was appointed to an instructorship at Harvard for 1881-82. During the summer of 1881 he travelled in Europe and was a visitor in Helmholtz's laboratory in Berlin long enough to complete measurements of the Hall effect in some new metals, which he reported to the York meeting of the British Association in the summer of the same year. In the fall of 1881 he returned to this country to begin his instructorship at Harvard. Here he served as instructor from 1881 to 1888, as assistant professor from 1888 to 1895, professor from 1895 to 1914, Rumford Professor from 1914 to 1921, and Emeritus Professor from 1921 until his death. In addition to the A.B. degree from Bowdoin and the Ph.D. degree from the Johns Hopkins, he received from Bowdoin the A.M. degree in 1878, probably the conventional degree awarded automatically at that time to all graduates of three years' standing, and in 1905 the honorary LL.D., also from Bowdoin. He was a member of the American Association for the Advancement of Science (vice president of the section of physics in 1904), of the American Physical Society, the National Academy of Sciences (elected in 1911), corresponding member of the British Association for the Advancement of Science, and foreign member of the Société Hollandaise des Sciences. He was a member of the Solvay Congress at Brussels in 1924, and of the Volta

Congress at Como in 1927. In 1937 he received the award and medal of the American Association of Physics Teachers for Notable Contributions to the Teaching of Physics, and was made the first honorary member of the Association.

He married, on August 31, 1882, Caroline Eliza Bottum of New Haven, Vermont, who had been his assistant at the Brunswick High School. She was descended on both her father's and her mother's (Hoyt) side from Simon Huntington who, tradition says, died at sea in 1633 while on his way from England to America with his family. A woman much beloved for her qualities of heart and mind, she died, after a brief illness, on June 1, 1921, just as his lectures ended before his retirement, and as he was looking forward to the cultivation of his leisure. There were two children, Constance Huntington Hall, who is now living in Cambridge, and Frederic Hilborn Hall, who died while a senior at Harvard. In January 1907 he suffered a severe nervous breakdown, probably associated with a physiological condition for which it was subsequently necessary to operate, and from which he did not recover sufficiently to resume his college work until the fall of 1908. In the spring of 1912 he was again subjected to a similar operation, made a rapid recovery, and was able to sail for England in July to attend the 250th anniversary celebration of the Royal Society of London as delegate of the American Academy of Arts and Sciences. During the summer he attended various scientific meetings, including the British Association meeting in Dundee. In 1914-15 he was again abroad, on sabbatical leave, and was in Italy when the World War broke out. Here he remained until early 1915, when he went with his family to France and gave lectures on the system of American education at Grenoble, Montpellier, Toulouse, and Bordeaux, under the auspices of the "Fondation Harvard".

Except for the illnesses mentioned above, he described himself as having been remarkably free from ordinary ailments. During college he was athletic, rowed on the college crew, and was always interested in sports, serving at one time on the Harvard Athletic Committee and writing an article on Athletic Professionalism and its Remedies. He played golf for fifteen years

after his retirement. His death on November 20, 1938, was due to heart failure, when he was apparently on the road to convalescence after an operation.

He always had a strong sense of civic duty, and in addition to his scientific activities engaged in many community enterprises. He was unobtrusively religious, and although never a regular church member, was a constant attendant at the First Congregational Church in Cambridge. From 1917-18 through 1927-28 he was president of the Family Welfare Society of Cambridge; in the autumn of 1919 he served as special police officer during the Boston police strike, being the first man to volunteer for service; in 1931 and 1932 he was chairman of the Cambridge Unemployment Relief Committee which raised through contributions from citizens over \$200,000 which was largely spent on relief projects, the most important of which was the construction of a nine-hole municipal golf course at Fresh Pond, Cambridge; from 1933 he was president of the Charles William Eliot Memorial Association, founded to commemorate, in 1934, the hundredth anniversary of Mr. Eliot's birth, and to work for a permanent Eliot memorial; and at the time of his death he was a member of the executive committee of the Cambridge Republican Council and a member of the Cambridge Community Federation.

Writing always remained a source of satisfaction to him. His prose style was perspicuous, and graced with a felicity that made it a pleasure to read. On occasion he was capable of effective verse. His scientific writings were not confined to technical expositions of his own research, but he wrote articles of popular appeal, and also on matters of education, in which he was always interested. During the later years of his life he was a frequent contributor to the "letter box" of the Boston Herald on current topics. After his retirement he found leisure to read classics in French, Italian, and Spanish; he was an omnivorous reader of the newspapers and popular weeklies. He was interested in paintings, and enjoyed informal music of his family and friends; he liked to go to movies but cared little for the modern theatre.

His technical contributions fall into two groups, of which the first is concerned with the teaching of elementary physics. When

he began teaching at Harvard in the fall of 1881 the admission requirements and the preparation given in the secondary schools were both largely perfunctory; there was practically no laboratory work, and students could even go through a college course of physics without performing an elementary experiment. The initiative in setting the requirement of laboratory work in physics for admission to Harvard came from President Eliot, who had worked as a chemist with Professor J. P. Cooke, said to have been the first man in America to have a laboratory for the teaching of chemistry. At the suggestion, and with the encouragement, of President Eliot, the young Hall prepared a set of 40 experimental exercises to be used by the secondary schools in preparing candidates for admission. This was issued in 1886 as the well known "Harvard Descriptive List of Elementary Physical Experiments." It had the great merit that the apparatus required was so simple and so well thought out that the preparatory school teachers were able to meet the requirements on their meager budgets, sometimes even constructing the apparatus with their own hands. The effect of the new admission requirements rapidly spread beyond the Harvard sphere of influence to that of the National Educational Association. Within a decade or two the leading manufacturers of apparatus were turning out complete sets of equipment for the experiments of the Descriptive List, which they had renamed the National Physics Course. It would not be unfair to describe this work of Professor Hall as entirely remolding the scheme of secondary school physics, and as such exerting a most important influence.

The "list" was followed by a number of text books of elementary physics, given in the bibliography, of which perhaps the best known is the "Hall and Bergen," published in 1891. The importance of his contributions to the teaching of elementary physics was fittingly recognized by the American Association of Physics Teachers at the 1937 Christmas meeting of the American Association for the Advancement of Science at Indianapolis, as has already been mentioned. The matter has been dealt with at length in the February 1938 issue of the *American Physics Teacher*. The effectiveness of his personal teaching was not due to any characteristics of brilliancy, but rather to a very real con-

cern that the student should grasp the situation, and to a meticulous setting forth of the difficulties.

The number of his research students was not large, perhaps not more than a dozen altogether. He took a keen personal interest in these, and there was a relation of genuine affection between them. In 1906, after 25 years' service at Harvard, he was presented with a silver loving cup, with the names of ten of his students, and the following inscription:

To Edwin Herbert Hall  
From His Research Students  
In Testimony  
Of Their Esteem and Gratitude  
In Appreciation  
Of His Work in the Field of Discovery  
Of His Quarter Century Service  
In Behalf of Harvard University  
His Life and Inspiration  
1881-1906

Of course Professor Hall will always be best known for the "Hall" effect, and the major part of his scientific activity was connected either directly with this effect, or else with closely related phenomena. The effect was discovered during his graduate work at the Johns Hopkins, and was the subject of his thesis. The effect is usually described as a difference of potential which appears perpendicular both to the lines of flow of an electric current and a magnetic field, the magnetic field itself being perpendicular to the current. He states, in his description of his discovery, that he was stimulated to search for the effect by a passage in Maxwell's *Electricity and Magnetism*: "It must be carefully remembered that the mechanical force which urges a conductor carrying a current across the lines of magnetic force acts, not on the electric current, but on the conductor which carries it.—The only force which acts on electric currents is electromotive force." This statement seemed to Hall to be "contrary to the most natural suppositions in the case considered." He

found an article by Professor Edlund on Unipolar Induction in *Philosophic Magazine* for October, 1878, which supported his feeling that there was trouble with Maxwell's conception, and he was encouraged to start his quest for the suspected effect by the remark of Rowland that he doubted the truth of Maxwell's statement, and had himself made a hasty experiment to detect some action of a magnetic field on a current, but with negative results. In the first experimental set-up, after "important changes in the proposed form and arrangement of the apparatus" had been made by Rowland, search was made for an increase of resistance in a wire carrying a current when a magnetic field was applied, which was thought would be the result of a suspected crowding together of the lines of current flow under the action of the field. The result of this first attempt was negative. Hall next searched for a transverse difference of potential, the arrangements being practically the same as in present arrangements for measuring the effect, and being also the same as had been previously tried by Rowland, but again with negative results. The reason for this failure we now know was merely lack of sensitivity, primarily due to too great thickness of the conductor. At Rowland's suggestion he tried again with gold leaf to carry the current, thus increasing the sensitivity by increasing the current density, and at once was rewarded with a positive result.

The effect appears to have been vaguely predicted by Kelvin, in 1851, who indicated the possibility of a term in his equations corresponding to the effect. There were at least three previous unsuccessful attempts to discover the effect: by Feilitzsch, Mach, and Gore. Wiedemann, in his "Galvanismus" described an experiment to prove that the effect does not exist, with apparatus practically identical with that used later apparently independently by Rowland, and with that with which the effect was eventually discovered.

Although an effect had been anticipated and searched for, and although the final discovery by Hall must be recognized to have been the reward of that persistence which was one of his most prominent characteristics, the effect actually found was essentially different from that anticipated. The effect anticipated was connected with the ponderomotive force acting on a conductor

carrying a current. It seemed almost necessary that the primary action responsible for the ponderomotive force on a conductor carrying a current in a magnetic field should be between the current and the magnetic field, the action on the current then getting propagated to the material of the conductor by some sort of secondary action. In fact, this primary action is explicitly written down in the equations of the Lorentz electron theory as a force on an electron moving in a magnetic field. This simple picture was definitely present in the minds of many of the early experimenters, and led to serious misconceptions as to the nature of the effect. Thus there were many attempts to detect a distortion of the lines of current flow and a consequent change of the effective resistance, as in Hall's original unsuccessful attempt. The impossibility of maintaining the original simple picture was obvious to all when presently metals were found in which the sign of the effect was different from that to be expected from the simple picture. It is an interesting coincidence that most metals, including those first measured, do have the expected sign. It would appear that we have here another example, of which there are not a few in the history of physics, of an effect suspected, obstinately searched for, and presently discovered, on the basis of a picture which has proved not to be pertinent.

It is my personal opinion that the implications of this discovery have not even yet been adequately appraised, even in connection with the mere methodology of the concepts with which we describe electrical phenomena in conductors. Certainly the program envisaged by Rowland in commenting on the young discovery has not yet been fully carried out: "The recent discovery by Mr. Hall of an action of magnetism on electric currents opens a wide field for the mathematician, seeing that we must now regard most of the equations which we have hitherto used in electromagnetism as only approximate", and even as late as 1915 there were most serious misconceptions of the nature of the effect in the writings of physicists of very high rank.

Hall's thesis contained a quantitative measurement of the effect in gold for various current strengths and for different values of the magnetic field, from which an approximately constant coefficient could be calculated. Even at the time of the

writing of his first papers he still hoped to be able to devise an experiment in which the lines of flow should be actually displaced in the magnetic field, but it was not long before he came to the accepted view of the current flowing obliquely to the equipotential surfaces under the action of the magnetic field. How little he realized the full implications of his discovery is suggested by the closing sentence of his first paper: "To make a more complete and accurate study of the phenomenon described in the preceding pages, availing myself of the advice and assistance of Professor Rowland, will probably occupy me for some months to come."

It has already been mentioned that in the summer of 1881 he travelled in Europe, and made a number of measurements of the new effect in Helmholtz's laboratory in Berlin. These measurements he reported in a paper read at the York meeting of the British Association in 1881. This paper was received with the greatest interest. Of it Kelvin said: "The subject of the communication is by far the greatest discovery that has been made in respect to the electrical properties of metals since the times of Faraday—a discovery comparable with the greatest made by Faraday." A large number of other investigators rushed into the field thus opened. The three other analogous transverse effects bearing the names of Ettingshausen, Nernst, and Righi-Leduc were speedily discovered, and a very extensive literature rapidly grew up.

Professor Hall himself for the next eight years continued to contribute papers dealing with measurements of the effect in various metals and with the proper way of conceptualizing the effect. From 1888 to 1891 there was a gap in his scientific publications. Except for a single paper on the temperature coefficient of the Hall effect in 1893 he did not return to the Hall effect until 1911. During this interval of 23 years his more serious papers were concerned with various thermal phenomena, such as thermal conductivity of metals, the thermodynamic behavior of liquids, and various thermo-electric effects, in particular, the Thomson effect. His principal graduate instruction was on kinetic theory and thermodynamics, and the direct connection between his lectures and his research activity is evident. He was particularly concerned to understand the mechanism of thermo-

electric action; among other things he showed that the conventional thermo-electric diagram of Tate is really a temperature-entropy diagram. He made important use of analogies with ordinary mechanical action, and sought to understand a thermo-electric current as something similar to a convection current in an unequally heated hydraulic circulatory system in a gravitational field. His vice-presidential address to the section of physics of the American Association for the Advancement of Science in December, 1904, was on "A Tentative Theory of Thermo-Electric Action." This theory he later modified, completely discarding certain features of it. In fact, among his papers was found a copy of his address with this writing in his own hand: "This was a very unhappy performance. I am sorry it was ever published." He always insisted, however, on the similarity to convective action and on the importance of the analogy. In 1911, in his paper with L. L. Campbell, he returned to the measurement of not only the Hall effect, but of the three other transverse effects as well. He was depressed by the disagreement between the values of the coefficients as measured by different observers, and was convinced that satisfactory values could be obtained only from measurements all on the same material, and if possible all on the identical piece of metal and in the same apparatus. This paper with Campbell was the first attempt at a realization of this ambition. For the rest of his life his experimental program was devoted to the accurate measurement of the four transverse effects under identical conditions, so that the values might be confidently used in theoretical speculations. In 1925 he published values for the coefficients of gold, palladium, cobalt, and nickel. After the erection of the New Research Laboratory of Physics at Harvard in 1931 he again attacked this problem with a reconstructed and improved apparatus. He was at work on this within a few weeks of his death, and had completed measurements of the four transverse effects as a function of temperature in copper and palladium, which apparently satisfied his exacting requirements. [The measurement of these coefficients is admittedly one of the most difficult tasks which an experimental physicist can set himself.] The delicacy of the measurements is suggested by the fact that one

of the effects which Professor Hall had to consider and eliminate in his last apparatus was convection currents in the air created by the magnetic field because of the slight paramagnetism of oxygen.] The dauntlessness of his experimental attack on this problem was characteristic of the man.

From 1917 on Professor Hall was occupied even more by his attempts to attain a theoretical understanding of electrical phenomena in metals than by his measurements. It was early evident to him from his theoretical attacks on the problem of thermoelectric phenomena that the little considered transverse effects could not be neglected in understanding the mechanism in metals, and his theorizing may be broadly characterized as an attempt to create a single whole which should include the thermoelectric and the transverse phenomena as well as those more usually considered. In commenting on a paper which he had just written for the Solvay Congress in 1924 he wrote: "Thus far the various transverse effects mentioned have been little more than a puzzle in science—things or phenomena to be explained. My hope is that through the paper I have just written I shall make them contribute to a luminous theory of the mechanism of metallic conduction, electrical and thermal." Perhaps the one outstanding feature that distinguishes his theory from others was his constant insistence on the importance of the rôle of the positive ions in metallic conduction. The early electron theories were concerned almost entirely with the rôle of the free electrons. It was obvious enough that where there are free electrons there must be ions, but for some reason the rôle of these ions was not explored in the conventional theories, perhaps because of mathematical difficulties. In particular, Professor Hall saw and insisted on the importance of the positive ions in affording an explanation for the unexpected sign of the Hall effect in some metals, and it was a great gratification to him toward the end of his life that the wave theory of metallic conduction fits the positive ions naturally into the complete picture, and shows their importance under conditions which he had anticipated.

His theoretical work probably has not exerted the influence which it otherwise might because it was written in his own peculiar idiom. He was not a mathematician, but he had a very

strong physical sense, which was most at home in a mechanistic medium very similar to that of the great English physicists. For him a theory consisted in a painstaking working out by native wits of all the consequences which he could see were inherent in the fundamental physical picture. The quantitative relations, whose existence he discovered in this way, were then thrown into mathematical form through the medium of power series or other conventional mathematical functions, the coefficients of which had only an empirical significance. The result was a mathematical edifice which had no organic connection with the underlying physical ideas; it would have been impossible to reconstruct the physical picture from the mathematics. The more conventional and usual course in constructing a theory is to formulate in mathematical terms the underlying physical picture, and then to allow the consequences of this picture to develop themselves by the more or less automatic functioning of the mathematical machinery. Professor Hall's procedure was bound to appear strange and uncongenial to the conventionally schooled theoretical physicist, with the result that the real merits of his basic physical ideas were too easily overlooked.

Several times during the last ten years of his life Professor Hall was tempted to collect the substance of his papers dealing with different aspects of his theory into book form, and in fact at one time he had a manuscript ready for publication. Finally, in the summer of 1938, he published through the Murray Printing Company, of Cambridge, Massachusetts, "A Dual Theory of Conduction in Metals", a book of 170 pages, much shorter than he had at one time contemplated. This book is, I believe, much easier to read than the original papers, and is worthy of intensive study. It should not be too difficult to put into the language of wave mechanics the essential features of his theory, and in at least some cases it is obvious that if this were done the two theories would have identical aspects. Wave mechanics has not even yet worked out the details of all the complicated effects in metals, and in these cases the insight afforded by his point of view may well be most valuable. In particular, it is to be mentioned that by his theory he was able to calculate the Nernst and the Righi-Leduc coefficients for a couple of metals from other

experimentally determined coefficients, an achievement which wave mechanics is hardly yet in a position to claim.

The mental characteristics to which he owed his success were probably first and foremost a certain obstinate methodicalness and clearness of apprehension. He writes of himself: "I am in some respects distinctly handicapped in all my scientific endeavors, being unskilful of hand and slow of apprehension. On the other hand, I am very persistent, and fond of wrestling with a difficult problem in my own slow way; any success I may have attained is to be attributed to these two qualities."

Those who knew him personally feel a loss greater than can be accounted for by any scientific eminence. It will be agreed, I believe, that his outstanding personal characteristic was his utter honesty and integrity, coupled with an independence and strength of character which enabled him to trust his own judgment, and steer his own course, once he had made his carefully reasoned decision. Combined with this was a very unusual reluctance to force his own views on others; he truly treated the opinions of others as equally worthy of respect with his own. Those who knew him more intimately knew that he had passed through dark times of discouragement or even despair, over which he triumphed by sheer force of character. Sometimes they were permitted glimpses of a depth and quality of sentiment rare and moving. His friends will not soon forget the erect vigor of his old age, or cease to be thankful that his last illness was not protracted.

BIBLIOGRAPHY

This bibliography is intended to include only the articles of scientific interest. It is as nearly complete as it has been feasible to make it, but there is no assurance that every such title has been included.

Key to Abbreviations

- Amer. Assn. Adv. Sci. Proc.—American Association for the Advancement of Science Proceedings  
Amer. Inst. Elec. Engrs.—American Institute of Electrical Engineers  
Amer. Journ. Math.—American Journal of Mathematics  
Amer. Journ. Sci.—American Journal of Science  
Amer. Phys. Teacher—American Physics Teacher  
British Assn. Adv. Sci.—British Association for the Advancement of Science  
Bull. Amer. Math. Soc.—Bulletin, American Mathematical Society  
Educ. Rev.—Educational Review  
Harvard Grad. Mag.—Harvard Graduates Magazine  
Journ. Phys. Chem.—Journal of Physical Chemistry  
Nat. Acad. Sci. Biog. Mem.—National Academy of Sciences, Biographical Memoirs  
Nuov. Cim.—Nuovo Cimento  
Phil. Mag.—Philosophical Magazine  
Phys. Rev.—Physical Review  
Proc. Amer. Acad.—Proceedings, American Academy of Arts and Sciences  
Proc. Nat. Acad. Sci.—Proceedings, National Academy of Sciences  
Sch. Sci. and Math.—School Science and Mathematics  
Sci.—Science  
Sci. Mo.—Scientific Monthly

1879

- On a new action of the magnet on electric currents. *Amer. Journ. Math.* II, pp. 287-292.

1880

- On Boltzmann's method for determining the velocity of an electric current. *Amer. Journ. Sci.* (3) 20, pp. 52-54. *Phil. Mag.* (5) 10, pp. 136-138.  
On the new action of magnetism on a permanent electric current. *Amer. Journ. Sci.* (3) 20, pp. 161-186. *Phil. Mag.* (5) 10, pp. 301-328.  
On a new action of the magnet on electric currents. *Amer. Journ. Sci.* (3) 19, pp. 200-205. *Phil. Mag.* (5) 9, pp. 225-230.

1881

- On the "Rotational Coefficient" in nickel and cobalt. *Phil. Mag.* (5) 12, pp. 157-172.  
The rotational coefficients in various metals. *British Assn. Adv. Sci., Reports*, pp. 552-553.

1883

- "Rotational Coefficients" of various metals. *Amer. Journ. Sci.* (3) 25, pp. 215-219. *Phil. Mag.* (5) 15, pp. 341-346.  
 Variations in the vertical due to elasticity of the earth's surface. *Sci.* 1, pp. 10-11.  
 Apparent attractions and repulsions of small floating bodies. *Sci.* 1, pp. 335-336.  
 Magneto-motive force. *Sci.* 1, pp. 391-392.  
 Auroral experiments in Lapland. *Sci.* 2, pp. 819-20.

1884

- Reply to "An Explanation of Hall's Phenomenon." *Sci.* 3, p. 387.  
 Inertia. *Sci.* 3, pp. 482-483, 562.  
 The Hall Effect (correction to abstract of paper on Hall Effect before American Physical Society, *Sci.* 4, p. 297, 1884). *Sci.* 4, p. 351. Letter to the editor.

1885

- The Hall Effect. *Sci.* 5, p. 249. Letter to the editor.

1886

- On the rotation of the equipotential lines of an electric current by magnetic action. *Amer. Journ. Sci.* 29, pp. 117-135. *Phil. Mag.* (4) 19, pp. 419-438. *Nuov. Cim.* (3) 19, p. 261.

1887

- Inertia-force. *Sci.* 9, pp. 158-159; 231-232. Letter to the editor.

1888

- Experimental physics for schools. *Sci.* 10, pp. 129-130. Letter to the editor.  
 Experiment on the effect of magnetic force on the equipotential lines of an electric current. *Amer. Journ. Sci.* (3) 36, pp. 131-146; 277-286.

1891

- A thermo-electric method of studying cylinder condensation in steam-engine cylinders. *Amer. Inst. Elec. Engrs.* May 20. 10 pp.

1892

- On the thermal conductivity of cast iron and of cast nickel. *Proc. Amer. Acad.* 27, pp. 262-270.

1893

- On a thermo-electric method of studying cylinder condensation in steam-engines. *Proc. Amer. Acad.* 28, pp. 37-50.  
 Note on an approximate trigonometric expression for the fluctuations of steam temperature in an engine cylinder. *Proc. Amer. Acad.* 28, pp. 51-54.

On the variations of the "Hall Effect" in several metals with changes of temperature. (With Albert L. Clough.) *Proc. Amer. Acad.* 28, pp. 189-199.

Teaching elementary physics. *Educ. Rev.* 4, pp. 157-163, 1892; 5, 325-333.  
Grammar-school physics. *Educ. Rev.* 6, pp. 242-248.

1896

On the thermal conductivity of mild steel. *Proc. Amer. Acad.* 31, pp. 271-302.

1898

Note on the experiments of I. Bosi on the resistance of moving electrolytes. *Phys. Rev.* 7, pp. 246-247.

1899

On the thermal conductivity of cast iron. (With C. H. Ayres.) *Proc. Amer. Acad.* 34, pp. 283-308.

On the relation between pressure and evaporation. *Journ. Phys. Chem.* 3, pp. 452-456.

Review of Burbury's *Kinetic Theory of Gases*. *Sci.* 10, pp. 685-688.

1900

Concerning thermal conductivity of iron. *Phys. Rev.* 10, pp. 277-310.

On the thermal and electrical conductivity of soft iron. *Proc. Amer. Acad.* 36, pp. 121-141.

1901

Mounting for tube containing liquified CO<sub>2</sub>. *Phys. Rev.* 12, pp. 125-126.

1902

Color-seeing. *The Nation*, September 18, 1902. Letter to the editor.

Buckingham's *Thermodynamics*. *Bull. Amer. Math. Soc.* 9, pp. 173-175.

1903

C<sub>v</sub> in liquids and the  $\alpha$  of Van der Waals in the case of water. *Phys. Rev.* 17, pp. 122-123; *Sci.* 17, p. 939; *Phys. Rev.* 16, p. 375, 1903 (Title only).

Do falling bodies move south? I. Historical, II. Method and results of the author's work. *Phys. Rev.* 17, pp. 179-190, 245-254.

On Van der Waals'  $a$  in alcohol and ether. *Sci.* 18, p. 663; *Phys. Rev.* 17, p. 499 (Title only).

1904

Experiments on the deviations of falling bodies. *Proc. Amer. Acad.* 39, pp. 339-349.

A tentative theory of thermo-electric action. *Sci.* 21, pp. 81-90.

The Van der Waals  $a$  in alcohol and in ether. *Boltzmann-Festschrift*, pp. 899-903.

Physics and chemistry in Prussian and American schools. *Educ. Rev.* 27, pp. 144-159.

1905

A tentative theory of thermo-electric action. *Amer. Assn. Adv. Sci. Proc.* 54, pp. 3-18.

A direct measurement of the Thomson thermoelectric coefficient in soft iron. *Phys. Rev.* 20, pp. 173-174.

Measurement of various thermal and electrical effects, especially the Thomson Effect, in soft iron. (With E. P. Churchill, L. L. Campbell, S. B. Serviss.) *Proc. Amer. Acad.* 41, pp. 23-55.

Experimental work in high-school physics. *Educ. Rev.* 30, pp. 94-96.

Natural science instruction. *Educ. Rev.* 30, pp. 396-414, 507-517.

1906

On thermo-electric heterogeneity in certain alloys, especially German-silver. (With L. L. Campbell and S. B. Serviss.) *Proc. Amer. Acad.* 41, pp. 543-574; *Phys. Rev.* 22, pp. 246-247.

Modern trend of physics teaching. *Educ. Rev.* 32, pp. 94-97.

1907

On the Thomson-effect and the temperature-coefficient of thermal conductivity of soft iron between 115° and 204° C. (With L. L. Campbell, S. B. Serviss, and E. P. Churchill.) *Proc. Amer. Acad.* 42, pp. 597-626.

1909

Relations of colleges to secondary schools in respect to physics. *Sci.* 30, pp. 577-586.

Measurement of surface tension. *Sch. Sci. and Math.* 9, pp. 759-762.

1910

Air-resistance to falling inch spheres. *Proc. Amer. Acad.* 45, pp. 379-384.

The teaching of elementary physics. *Sci.* 32, pp. 129-146.

1911

On the electromagnetic and thermomagnetic transverse and longitudinal effects in soft iron. (With L. L. Campbell.) *Proc. Amer. Acad.* 46, pp. 625-668.

Metals on metals, wet. *Sci.* 33, p. 775. Letter to editor.

1912

Sailing flight of birds. *Nature* 90, pp. 161-162. Letter to editor.

1914

On electric conduction and thermoelectric action in metals. *Proc. Amer. Acad.* 50, pp. 67-103.

Experiments to illustrate the stability of ships. *Sch. Sci. and Math.* 14, pp. 684-685.

Aerodynamic paradox. *Sch. Sci. and Math.* 14, pp. 686-687.

1915

Una Possibile Spiegazione del Fenomeno di Hall e del Fenomeno di Ettingshausen. *Nuov. Cim.* (6) 9, pp. 5-12.

1916

Notes sur L'Enseignement aux Etats-Unis. *Revue Internationale de L'Enseignement* 70, pp. 343-356, 443-456, 1916; 71, 34-46, 1917.

1917

A possible function of ions in the electric conductivity of metals. *Proc. Nat. Acad. Sci.* 3, pp. 163-171.

1918

The validity of the equation  $P = TdV/dT$  in thermo electricity. *Proc. Nat. Acad. Sci.* 4, pp. 11-13.

Thermo-electric diagrams on the P-V-Plane. *Proc. Nat. Acad. Sci.* 4, pp. 29-35.

Thermo-electric action with dual conduction of electricity. *Proc. Nat. Acad. Sci.* 4, pp. 98-103.

Thermo-electric action with thermal effusion in metals: A correction. *Proc. Nat. Acad. Sci.* 4, pp. 297-299.

Theory of thermal conductivity in metals. *Phys. Rev.* 11, pp. 329-330.

1919

The thermo-electric equation  $P = TdV/dT$  once more. *Proc. Nat. Acad. Sci.* 5, pp. 197-198.

1920

Inferences from the hypothesis of dual electric conduction; The Thomson Effect. *Proc. Nat. Acad. Sci.* 6, pp. 139-154.

The Thomson Effect and thermal conduction in metals. *Proc. Nat. Acad. Sci.* 6, pp. 613-621.

1921

The Peltier Effect. *Proc. Nat. Acad. Sci.* 7, pp. 62-66, 1921.

Thermo-electric action and thermal conduction in metals: A summary. *Proc. Nat. Acad. Sci.* 7, pp. 98-107.

1922

An electron theory of electric conduction in metals. *Proc. Nat. Acad. Sci.* 8, pp. 307-313.

1923

A theory of the Hall Effect and the related effect for several metals. *Proc. Nat. Acad. Sci.* 9, pp. 41-46.

The quasi equation  $P = TdV/dT$ . *Proc. Nat. Acad. Sci.* 9, pp. 207-211.

1924

Metallic conduction and the transverse effects of the magnetic field. *Report Solvay Conference of 1924*, pp. 303-349.

The number of free electrons within a metal. (For Members of the Fourth Solvay Conference.) Cambridge, July 7, 1924, 4 pp.

Contact potential difference of metals. Privately printed, August 7, 1924, 6 pp.

1925

The number of free electrons within a metal. *Proc. Nat. Acad. Sci.* 11, pp. 36-38.

Conditions of electric equilibrium at boundary surfaces; Volta Effect. *Proc. Nat. Acad. Sci.* 11, pp. 111-116.

The four transverse effects and their relations in certain metals. *Proc. Nat. Acad. Sci.* 11, pp. 416-422.

Measurement of the four magnetic transverse effects. *Phys. Rev.* 26, pp. 820-840.

1926

New evidence in favor of a dual theory of metallic conduction. *Proc. Nat. Acad. Sci.* 12, pp. 326-328.

Illustrations of the dual theory of metallic conduction. *Phys. Rev.* 28, pp. 392-417.

Note on the temperature relations of photo-electric emission and thermionic emission of electrons. *Proc. Nat. Acad. Sci.* 12, pp. 486-488.

Sand flotation. *Sci.* 63, p. 571.

1927

Photo-electric emission, thermionic emission and Peltier Effect. *Proc. Nat. Acad. Sci.* 13, pp. 43-46.

Thermionic emission and the "Universal Constant" A. *Proc. Nat. Acad. Sci.* 13, pp. 315-326.

Ball dancing on water-jet. *Sci.* 65, p. 300.

Volta's theory of the Volta Effect: A modern interpretation. *Estratto dagli Atti del Congresso Internazionale dei Fisici.* Como- II, pp. 149-158, Settembre 1927.

1928

The Fermi statistical postulate; Examination of the evidence in its favor. *Proc. Nat. Acad. Sci.* 14, pp. 366-370.

Sommerfeld's Electron-Theory of metals. *Proc. Nat. Acad. Sci.* 14, pp. 370-377.

Electron "Free Path" and supra-conductivity in metals. *Proc. Nat. Acad. Sci.* 14, pp. 377-380.

Electric conductivity and optical absorption of metals. *Proc. Nat. Acad. Sci.* 14, pp. 802-811.

1929

Photo-electric emission and thermionic emission once more.: *Proc. Nat. Acad. Sci.* 15, pp. 126-127.

On electrons that are "Pulled Out" from metals. *Proc. Nat. Acad. Sci.* 15, pp. 241-251.

Further remarks concerning thermionic "A" and "b," a revision and extension. *Proc. Nat. Acad. Sci.* 15, pp. 504-514.

1930

Recent progress in the dual theory of metallic conduction. *Proc. Nat. Acad. Sci.* 16, pp. 45-55.

The "Reaction-Isochore" equation for ionization within metals. *Proc. Nat. Acad. Sci.* 16, pp. 371-373.

1931

Electric conductivity and optical absorption in metals, once more. *Proc. Nat. Acad. Sci.* 17, pp. 392-401.

Electric conductivity and optical absorption in metals (Supplementary). *Proc. Nat. Acad. Sci.* 17, pp. 427-430.

Michelson and Rowland. *Sci.* 73, p. 615.

1932

Action of a running horse. *Sci. Mo.* 35, pp. 466-469.

1933

On supraconductivity and the Hall Effect. *Proc. Nat. Acad. Sci.* 19, pp. 619-623.

1937

The four transverse effects in copper: new measurements. *Proc. Nat. Acad. Sci.* 23, pp. 600-603.

1938

Physics teaching at Harvard fifty years ago. *Amer. Phys. Teacher* 6, pp. 17-20.

The four magnetic transverse effects in copper and their changes with temperature. New measurements. *Proc. Amer. Acad.* 72, pp. 305-325.

*Abstracts of Papers read at Scientific Meetings not made by Professor Hall.*

The rotational coefficients in various metals. *Nature* 25, pp. 46-47, 1881.

Abstract of paper on Hall Effect read before American Physical Society. *Sci.* 4, p. 297, 1884.

The measurement of thermal conductivity in iron. *Sci.* 8, p. 530, 1898. (Abstract of paper read at Boston Meeting of A. A. A. S.)

On hydrogen-charged palladium. *Sci.* 19, p. 204, 1904. (Abstract of paper read at A. A. A. S., Feb. 5th.)

Books

Elementary ideas, definitions, and laws in dynamics. Riverside Press. Cambridge. 1886.

- Descriptive list of elementary exercises in physics, for admission to Harvard College, etc. Cambridge. 1886, 1892, 1901, 1903, 1912.  
 A text-book of physics. H. Holt and Company, New York. 1891, 1897, 1903. (With Joseph Y. Bergen.)  
 Elementary lessons in physics. H. Holt and Company, New York, 1894.  
 Elementary lessons in physics. H. Holt and Company, New York, 1900.  
 "May be regarded as 2nd edition of Elementary lessons in physics," from introduction.  
 A key to "Text-book of physics." H. Holt and Company, New York, 1894. (With Joseph Y. Bergen.)  
 The teaching of chemistry and physics in the secondary school. Longmans, Green and Company. 1902. (With Alexander Smith.)  
 Elements of physics. H. Holt and Company, New York, 1912.  
 Suggestions for teachers using Hall's "Elements of Physics." H. Holt and Company, New York, 1912.  
 College laboratory manual of physics. H. Holt and Company, 1904; Harvard University Press, 1913.  
 A dual theory of conduction in metals. Murray Printing Company, Cambridge, 1938.

*Biography*

- Benjamin Osgood Peirce, 1854-1914. *Nat. Acad. Sci. Biog. Mem.* 8, pp. 437-466, 1919.  
 John Trowbridge, 1843-1923. *Nat. Acad. Sci. Biog. Mem.* 14, pp. 185-204, 1931; *Harvard Grad. Mag.* May, 31, pp. 526-7, 1923.  
 Arthur Gordon Webster. *Sci.* 58, pp. 37-39, 1923; *Phys. Rev.* 21, pp. 585-586, 1923.  
 Wallace Clement Ware Sabine, 1868-1919. *Harvard Grad. Mag.* March 1919, 7 pp.; *Nat. Acad. Sci. Mem.* 21, 19 pp., 1926; *Proc. Amer. Acad.* 60, pp. 646-651, 1925.  
 Sketch of the life of James Clerk Maxwell, written in the centennial year of his birth. *Sci. Mo.* 33, pp. 500-511, 1931.  
 Augusto Righi (1850-1920). *Proc. Amer. Acad.* 69, No. 13, 1934.





*Graham Tusk.*

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OF

GRAHAM LUSK

1866–1932

BY

EUGENE F. DU BOIS

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## GRAHAM LUSK

1866-1932

BY EUGENE F. DU BOIS

The great life work of Graham Lusk was inspired by his father and by his teacher, Carl Voit in Munich. When Lusk returned to America he strove for an orderly development of the science of nutrition. His own discoveries, though of considerable importance in themselves, served chiefly in illuminating the whole field. In the light shed by his own experience he was able to judge the value of the work of others, interpret, synthesize. He was devoted to the science of nutrition and in time he brought order out of chaos. His work fell in the period of great developments in chemical energy transformation before the studies on the vitamins changed the whole trend of investigation. It does not matter how relatively important the vitamins may be. They do not in any way diminish the absolute importance of the metabolism of foods and the energy requirements.

Graham Lusk was admirably equipped to develop a field of science in our country. He was endowed with boundless enthusiasm, a fixity of purpose and a clear vision of his ideals in research and scientific education. For the attainment of these ideals he employed every resource. He was supported by his family traditions, his position in the social life, aided by his buoyant personality, his gifts as a lecturer. All of these were subordinate to the brilliant idealism that dominated his life and governed his success.

Lusk did not make discoveries in metabolism as important as those of Rubner, but it is doubtful if the importance of Rubner's discoveries would have been realized had it not been for Lusk. He never announced startling findings that set the scientific world agog, bringing a harvest of publicity and medals, but his quiet work often disproved those very experiments and theories that had won renown. Medals are given for the announcement of new discoveries. The man who shows the discoveries are wrong gets no medal; nothing but the satisfaction that he has helped to guide his group of fellow investigators out of a desert

beset with dangers. Lusk liked to set things straight and I doubt if any man enjoyed more heartily a good controversy. He would gird his loins and plunge into the fray, followed by his students who loved to watch him in combat. He fought fairly, retaining the friendship and admiration of his opponents.

It would be impossible to appreciate the career of Graham Lusk without a knowledge of his family background. The first of the family in this country was Stephen Lusk, who migrated from Scotland to Wethersfield, Connecticut, in 1702. The family was distantly related to Sylvester Graham, after whom Graham bread was named. The Lusks remained in Connecticut for many generations and the stock represents well the prosperous, educated natives of that state.

His grandfather was Sylvester Graham Lusk, his father, William Thompson Lusk, who was born in Norwich in 1838. His mother was Mary Hartwell Chittenden of New Haven, a thoughtful, cultivated, intellectual woman. Her father, Simeon Baldwin Chittenden, who married Mary Elizabeth Hartwell, was the seventh generation descended from William Chittenden, an original settler of Guilford in 1639. Simeon Chittenden was a prosperous merchant who lived most of his life in Brooklyn, an alert, aggressive man of great energy, conservative and charitable in his gifts. He served as Congressman for seven years.

Graham Lusk resembled his father, William T. Lusk, a practitioner of medicine, several generations ahead of his time. William T. Lusk went to Yale for a year, but left to study chemistry and physiology for two years in Heidelberg and one year in Berlin. In 1861 he rushed back to this country and volunteered in the army where he was in the thick of the fighting until 1863, retiring with the rank of captain and assistant-adjutant general. His war letters (1)\*, published privately by his family, are written with humor and vigor and make excellent reading. After leaving the army he studied medicine at the Bellevue Hospital Medical College, an institution in which he, and later his son, taught for many years. Soon after his graduation he married and went abroad for study in Edinburgh, Vienna, and Prague. When he

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(1)\* This and similar numerical references are to the titles listed under "Other References" and "Tributes" at the end of the memoir, pages 141 and 142.

returned, he practiced medicine for a year in Bridgeport, Connecticut, and it was in this city that Graham was born. In 1866 he returned to New York where he practiced medicine, devoting particular attention to obstetrics. In 1869 he was made Professor of Physiology and Microscopic Anatomy in the Long Island College Hospital. In the winter of 1870-71, at the request of Dr. Oliver Wendell Holmes, he delivered a course of lectures on physiology at the Harvard Medical School (2), being the first lecturer in this subject who gave experimental demonstrations. This course was very successful and he was offered the chair by Harvard a few hours after he had accepted the professorship of Obstetrics and Diseases of Women and Children at the Bellevue Hospital Medical College. Although William T. Lusk became famous as an obstetrician, he never lost sight of his training in physiology. His well-known book, "The Science and Art of Midwifery," which appeared in 1882, was written with physiology as a background. It went through four editions and was translated into four languages, French, Italian, Spanish, and Arabic. The senior Lusk published many papers, the most prophetic being, "Origin of Diabetes with Some New Experiments Regarding Glycogenic Function of the Liver."

William T. Lusk practiced medicine from the standpoint of a man trained in physiology and research, well acquainted with the best laboratories and clinics in Europe. He was surrounded in New York by physicians of the old school, all but a few of these ignorant of the fundamental sciences. It was an era of bitter controversies and open warfare between the faculties of the various medical colleges. With his army training, he was a good fighter, sometimes impulsive, but just and magnanimous, and a generous antagonist. Graham Lusk, until the age of thirty-one, was in close touch with his father and always revered his memory. His example was constantly in his mind and he strove to reform medical education so as to give the students the training and point of view that had made his father a leader in his profession. Graham Lusk was as fearless as his father and fought just as strenuously in any cause that needed his support. He lived to see the medical profession in his city grow out of the belligerent attitude of his youth and he himself helped

to eliminate personalities and jealousies, and bring the discussion to a consideration of policies rather than politics.

Graham Lusk was born in Bridgeport, Connecticut, on February 15, 1866, the family moving to New York for permanent residence a few months later. His mother died when he was five years old, leaving three other children, Mary Elizabeth, who married Cleveland Moffett, William Chittenden Lusk (3), who became a prominent surgeon in New York, and Anna Hartwell Lusk, who is still living. His father married again in 1876 and the only child of this union, Alice, married Dr. John Clarence Webster.

Graham prepared for college at the Berkeley School. At the age of sixteen to seventeen he travelled in Europe. He entered the School of Mines, Columbia University, from which he received the degree of Ph. B. in 1887. On account of an increasing deafness, his father persuaded him to give up the idea of the practice of medicine and he went abroad, intending to study chemistry and physiology under Hoppe-Seyler. In Munich he visited von Winckel, the obstetrician who was a friend of his father. He had never heard of Carl von Voit until Winckel gave him a card of introduction, and it was a pure piece of luck that he became his pupil.

In a delightful address, "Carl von Voit—Master and Friend", delivered in 1930, Lusk describes his student days in Munich.

"My first interview with Voit was disappointing. I could not enter his laboratory for a year. I must first hear his lectures, taking his practical course (in which a dozen students stood up for two hours and watched the professor make experiments), and take anatomy under Rüdinger and histology under Kupfer. This was quite as it should have been, though it was hard for me to realize it at the time.

"Voit's lectures were a delight to me. He read them from a text. If he wished to interpolate matter there was no change in the smoothness of delivery; he spoke as though the new ideas were being read from manuscript. He was very short-sighted and only on rare occasions raised his eyes from his manuscript. This happened if he heard a noise in the room; he stopped, glared at his audience, his eyes flashed fire, a few enraged words followed, and then the lecture went on as peacefully as though nothing had happened. The class never forgot the lesson; it

knew its master. True to the German form, the presentation of the scientific facts was always preceded by a short description of the historical development of the subject. . . .

"The Munich of the day of which I speak was a simple old-fashioned German town. The first summer that I spent there I lived on the Karlstrasse, having taken a room above a beer hall at five dollars a month. The good Frau who rented the room gave me a roll without butter and a cup of coffee in the morning for five cents. When my friend, I. N. Phelps Stokes, the man who has written of New York City the most notable history of a city ever produced, joined me for a month, a second bed was put in the room and the cost was raised by five marks a month. That made a rental charge for a furnished room of \$3.10 apiece a month. Out of the window we could see the Munich cab drivers who patronized the beer tap below and who ate the same coarse rye bread which they fed to their horses. . . .

"Professor von Winckel was notably kind to me. I went to his home to dinner often on Sunday noon when the unmarried and married children and the grandchildren gathered around a bountiful table. They were all my good friends. I attended a large entertainment there. I got into a dispute with a German about the relative beauty of German and American women. 'Ah,' said he triumphantly, 'the woman now entering is the most beautiful woman in the room. Have you any like her in America?' 'She is an American,' replied I. 'Impossible,' exploded the German. He turned to inquire of his neighbor and the argument stopped. It was Frau Hanfstengel, who had been a Miss Sedgwick, of Stockbridge, Massachusetts. . . .

"Munich, the Isar Athens, which had been the home of Liebig and Wagner, and was then the adopted home of Ibsen, Munich with its Frauenkirche, its distant view of the snow-covered mountains of the Tyrol, its companionable people, this was the true soul of Germany to me, and I sought to understand it and be absorbed by it.

"After a year of probation I entered Voit's laboratory, and in 1888 he gave me a problem on diabetes to investigate. Curiously enough, in 1871 in volume I of Maly's *Jahresbericht*, is recorded under the index head Diabetes an abstract of an article by my father. So in each decennium for sixty years articles bearing the family name have appeared on this subject.

"The professor having outlined the problem, his assistants gave me every necessary help. Herr Seidl, the town baker, prepared a batch of zwieback and then prepared crisp gluten bread which was free from carbohydrate. For the first period my daily diet consisted of coffee, steak, 500 grams of zwieback,

butter, and with each of the two main meals a pint of wine. After three days the zwieback was replaced by an amount of gluten bread, the nitrogen content of which exactly corresponded to that in the discarded zwieback. The 500 grams of zwieback daily gave me the subjective sensation of having deposits of glycogen in the brain, whereas the gluten bread in its turn tasted like wall paper. Dropping the carbohydrate in the zwieback, but continuing the nitrogen intake at its previous level by ingesting gluten bread, led to a great increase in protein metabolism and demonstrated the protecting power of carbohydrate over such metabolism. The article concerned with this work was written entirely by Voit and appeared in the *Zeitschrift für Biologie* under my name. When I remonstrated and told Voit that his name should be there also he replied, 'Da ist es—*"Aus dem physiologischen Institut zu München."*' Dr. Welch read the article at home and spoke to my father about it. I felt exalted.

"At Columbia my instructors had been of two types, gentlemen who were good teachers who did not know very much, and rather rough people who knew a great deal. In Munich, for the first time in my life, I had found a teacher who represented a highly developed form of culture which was both intellectual and personal. . . .

"The laboratory, with Wilhelm Prausnitz and Max Cremer as first and second assistants, was a happy, friendly place. One day I burned my hand with ether. To relieve the pain a servant was sent to buy cocaine which cost \$1.75. I offered to pay for it, but money was refused. I was told I had done so much for the state that state funds would care for me in this trouble. What had I done for the state? I had given sugars, including some levulose, at that time a rare sugar which I had prepared myself, to divers rabbits, and had analyzed their livers for glycogen and their intestinal tracts for sugar. This was a new conception of service for the welfare of the state. It determined my attitude that qualified workers who would give their time to research should be given every laboratory aid humanly possible. . . .

"Voit never attended scientific meetings or congresses except the meeting of the local scientific society, at which the members could take supper and at which all drank beer during the proceedings. Only through his *Zeitschrift für Biologie* did he rub shoulders with the world. Once shortly after a violent polemical dispute of his with Pflüger I happened to be in Bonn and met Pflüger through the influence of an American woman, the daughter of William Walter Phelps, one who had married Franz von Rothenberg, one of Bismark's lieutenants and who, retired, was then the financial agent of the Prussian government by whom

the professors were paid. Pflüger appeared to me to be German of the highest type and in many ways reminded me of Voit himself. He was willing to admit privately that sugar might arise from protein in metabolism, but in his literary productions in *Pflüger's Archiv* at the time he was violently against the proposition.

"When I saw Voit a few days later he was dumbfounded to learn that Pflüger had received me, knowing whose pupil I was. He told me I had been in the lion's den. When I said that Pflüger would admit the possibility of the production of sugar from protein he replied that the lion would soon become tame.

"Voit always manifested the greatest interest in a new discovery. The figures were 'sehr schön,' he would say. My colleague Jackson has poked fun at me for talking about 'a perfectly beautiful experiment.' I think this must be the unconscious translation of 'ein sehr schöner Versuch,' words so often used by Voit.

"Voit, when I first knew him, was fifty-five years old, medium in size, keen-eyed, alert, with a quick walk, of quiet, dignified, courteous bearing. He knew that he was the founder of modern metabolism research, and yet he was in many ways as simple as a child, as simple as a German scholar of the old school, I might have said. After I had been in Munich three years he, in greeting my father, held out both his hands, exclaiming, 'Und das ist der Papa.' He invited me to dinner with the words, 'We have said to one another we must have Herr Lusk meet us in the family, and you will come, will you not?' Of course I went. One of the daughters served the dinner. The first assistant in his laboratory had never been asked into his home. Later Mrs. Lusk and I were always invited to dine with the family whenever we went to Munich. . . .

"I have a few old letters which I have translated and which illustrate better than my own words the causes of Voit's influence over me. There is constant emphasis on scientific work."

Space permits the quotation of only one of these, written on the occasion of Voit's seventieth birthday.

"Munich, Sept. 26, 1901.

"Dear Colleague,

"You had the goodness and friendliness to remember my seventieth birthday by contributing to the marble bust given me by my pupils and also by writing a paper 'Ueber den Phlorhizindiabetes,' for the *Festschrift* of the *Zeitschrift für Biologie* which was dedicated to me. It was a great pleasure for me that my old students, who since 1863 have grown to a

great number, thus recognized my efforts and showed their devotion. You are one of my most faithful and most grateful pupils, and it gave me much pleasure to find you among those who participated. I have read with great interest your valuable contribution, especially your proof that sugar does not arise from fat in phlorhizin diabetes. From this I have again seen that you are independently capable of expanding the work which I began and of promoting science. I thank you affectionately for the love and the good which you have done me and I beg you furthermore to preserve your love for me.

“Ihr getreuer alter Lehrer

“CARL VOIT.”

Voit died on January 31, 1908, aged seventy-seven years. He suffered from a diabetes of long standing.

Lusk received the degree of Ph. B. in chemistry from von Baeyer in the University of Munich in 1891. He brought back to America all that was best in Voit's laboratory and with it a long heritage, almost an apostolic succession. One of his pupils dedicated a book on basal metabolism (4)

To

Graham Lusk

Pupil of Voit

Pupil of Liebig

Pupil of Gay-Lussac

Pupil of Bertholet and Laplace

Pupil of Lavoisier

Perhaps the most important thing that Lusk brought back from Germany was the memory of one sentence. The incident is described only in a brief autobiography, which according to his directions was to be confidential until his death—"About this time (1891) my father and I called on Voit in the Munich laboratory. Voit, in talking with him, recounted the celebrated names in German physiology, Du Bois-Raymond, Ludwig, and others; mentioned those of his own age, Heidenhain, Pflüger; stated that the quality of men was deteriorating, that Englemann, recently appointed at Berlin to succeed Du Bois-Raymond, was a very good physiologist but no proper successor to the chair formerly held by Johannes Müller, and that of promising men

of forty years of age there were none; then he added, 'perhaps your son will become one.' I recall these words because I was not at that time, at the age of twenty-five, conscious of having any capacity for planning any piece of creative work, and also to illustrate how the confidence of a beloved and admired teacher may stir the ambitions of a young man. I have never repeated this story to anyone."

Lusk's friends in Voit's laboratory were Max Cremer, Erwin Voit, Carl's brother, Fritz Voit, Carl's son, Otto Frank, and W. Prausnitz. He came to know well many other pupils of Voit, Max Rubner, Friedrich von Müller, W. O. Atwater and E. P. Cathcart. Except for a brief period during the war, Lusk kept up a constant correspondence with Germany and made many visits to the laboratories. Although he had numerous friends in all the countries of Europe, his closest contacts were with Germany and England.

When Lusk returned to America he was appointed instructor in physiology at Yale University, was advanced to assistant professor in 1892, and to professor in 1895. As head of this department he was given a very small salary, but fortunately did not depend on this as his grandfather S. B. Chittenden had, a few years previously, left him with sufficient means to relieve him from any early financial struggles. Russell H. Chittenden quotes a letter written by Lusk to a friend under date of February 2, 1931.

"When I was twenty-five years old I found myself the head of a department of physiology at a salary of \$300 per annum with an allowance of \$150 annually for apparatus. The department consisted of one room in the old building at 150 York Street. Here Dr. L. C. Sanford and I brought up some pigs on the bottle and here the phlorhizin work on rabbits was started. I did all the cleaning and I mopped up the floor myself. . . . Later a new laboratory building was built in the yard of which I had an entire floor . . . and at last I had someone to wash dishes. Here phlorhizin brought new information from dogs. . . . I never had an assistant while I was in New Haven. My salary and that of the other professors had been raised to \$500 before I left. . . . In New Haven my life was one of peace for seven years."

Life must have been pleasant in New Haven for he had many friends and enjoyed the opportunity for riding and driving. His reputation was increasing and it is not surprising that in 1898 he was called to the chair of physiology in the New York University and Bellevue Hospital Medical College in New York City, an institution with which his father, who died in 1897, had been long connected. On December 20, 1899, shortly after coming to New York, Graham Lusk married May Woodbridge Tiffany, daughter of Louis C. Tiffany, the artist and designer of glass. He was fortunate in a devoted wife who sympathized heartily with his ideals and enjoyed the academic life and the many visits to the research centers of Europe. Lusk's home at first at 11 East 74th Street was a center for physiologists of all nations and the dinner parties at this house did much to cement the bonds between the laboratories of our country and Europe. It was in Lusk's library that two important societies were formed, the Society for Experimental Biology and Medicine in 1903, and the Harvey Society in 1905. The Lusks took their vacations in the Adirondacks and later at their beautiful summer home at Syosset, Long Island, adjoining the large estate of Mr. Tiffany. There were three children, William Thompson Lusk, Louise, wife of Collier Platt, and Louis Tiffany Lusk.

Here in his native town, New York, Graham Lusk found greater opportunities, better equipped laboratories, and the stimulating task of instructing large numbers of students. In an address, "Scientific Medicine—Yesterday and Tomorrow", written in 1919, he describes the status of clinical medicine at the end of the last century. He quotes a letter from his life-long friend, Theodore Janeway, then twenty-seven years old. The younger Janeway had hopes that in coming to teach at the Bellevue school "he could take some part in the advance of true medical knowledge and not merely diffuse what is already known." He concludes his letter:

"I trust it will be possible for me to keep in touch with your work and Dr. Dunham's next winter and especially with your enthusiasm, and that I may be able to persuade the students to

regard symptomatology as the physiology of the 'sick life'. It will certainly be a most interesting experiment from my side."

Lusk goes on to say:

"This letter gives no picture of New York medicine as it was then. One should remember that Meltzer, at the time, was a practicing physician, snatching such moments of rapture as were his when he drove down Fifty-Ninth Street, tied his horse to a lamp post near the P. and S. and, with his coachman who acted also as laboratory servant, entered the dingy recesses of that college and ascended to the laboratory of physiology, there to perform some fundamental experiment, perhaps one on the nature of shock, for example. At that time Herter had just undergone metamorphosis from a specialist in nervous diseases into a physiologic chemist. Park had established a modern laboratory in the board of health. But besides this there was little to encourage the adventurer into clinical science. As matters of fact be it recorded that a few years later the medical faculty to which Janeway was attached refused him a teaching position in Bellevue Hospital, though the salary belonging to the position had been raised from outside sources, and the same faculty held a solemn, special meeting to discipline me because I had publicly before the students expressed opinions favorable to the Johns Hopkins Medical School.\* These were days when there appeared to be no future for clinical science, days in which there was almost no intellectual, social or financial influence making for its welfare. And yet we in this country, since that time, have made great headway in this direction, not on account of the influence of any special men, but because the principle that the primary mission of a medical school, 'to take some part in the advance of true medical knowledge and not merely to diffuse what is already known,' is everlastingly right."

Graham Lusk must have been a vigorous crusader even in these early years. He was in his home town, surrounded by influential friends, steeped in the best traditions of German science. He was always fearless, ready to combat single-handed a great foundation or faculty of a medical college. One can picture his amusement and gratification when he was solemnly rebuked in a faculty meeting for having praised the work of a

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\* One of Lusk's contemporaries has pointed out that Lusk was impatient in his efforts to reform clinical medicine. He tried at times to push men when their teachings were so far above the heads of the students that they could not be appreciated. There were probably a good many factors in this protest from the faculty.

rival institution. He considered this one of the highest honors he had ever received.

At the Bellevue Medical College he was happy in his association with Theodore Janeway, E. K. Dunham, W. H. Park, George B. Wallace, John Mandel and Arthur R. Mandel. He was perhaps unhappy in the attitude of the heads of the clinical departments of his own and other medical colleges who were products of the old school. Fortunately he had good assistants, P. G. Stiles, A. R. Mandel, A. I. Ringer and John R. Murlin. He secured a small Pettenkofer-Voit respiration apparatus for the study of dogs with phlorhizin diabetes. He continued his work on carbohydrates, employing as a tool his discovery in New Haven, that in the glycosuria produced in dogs by the administration of the drug phlorhizin the D:N ratio was 3.75. In other words, the dogs excreted in the urine 3.75 grams of dextrose for every gram of urinary nitrogen. Since one gram of nitrogen in the urine indicated that the dog had metabolized 6.25 grams of protein, this meant that a little more than half a gram of carbohydrate could be formed from the metabolism of one gram of protein. Later he found that different amounts of dextrose were derived from the different amino acids which constitute the protein molecule. It required extreme care to secure complete phlorhization of the dogs and exclude the dextrose derived from stolen food or glycogen stored in the liver. Lusk's whole life was plagued by experimenters whose phlorhizin technique was faulty.

Some of the work published in this period was so far in advance of its time that it was neglected. For example, in a paper with Mandel, published in 1903, he said, "The calories lost in urinary sugar in diabetes are compensated for by the increased proteid metabolism. In a diabetic dog, whether he be fasting or fed on meat alone or on fat alone or on meat and fat together, no more fat is burned than in the same dog when he is normal and fasting."

This was a fundamental discovery neglected by practically everyone except Lusk and his pupils. The literature on diabetes for many years was filled with calculations based on the food consumed, not the food metabolized. It was about ten years

later that others began to grasp this idea. E. P. Joslin, at the memorial meeting to Graham Lusk held in 1932 (23), through a natural error, mistakenly ascribes this concept to one of Lusk's pupils working on human diabetes under Lusk's direction. Joslin says: "He who runs may read that, despite all your elaborate formulae, it is not what a diabetic patient is supposed to eat or does eat, but what the calorimeter proves that he burns which counts. Any doctor who has once fully grasped that idea will make few errors in the dietetic treatment of his diabetic patients."

In another paper describing the influence of cold and moderate exercise on the sugar excretion in phlorhizin glycosuria, in 1908 Lusk says: "It is therefore apparent that an amount of work capable of more than doubling the fat metabolism has no effect whatever on the sugar output in a case of total phlorhizin glycosuria. Hence sugar is not derived from fat in metabolism." Further on he says, "This discussion does not exclude the possibility that after *large* fat ingestion a certain quantity of dextrose may be formed from the quickly absorbed glycerine component of fat. But this result has never been seen in this laboratory." Lusk held this opinion until his death. He and all his pupils fought vigorously the theory that in diabetes glucose could be formed from the metabolism of fatty acids.

In another paper on phosphorus poisoning in dogs (1907) fever was produced and the metabolism was raised "perhaps on account of the fever and perhaps on account of the specific dynamic action of the increased protein metabolism."

At this early period Lusk had established the main facts in the metabolism of diabetes and fever. He had investigated with Mandel a case of human diabetes of maximum severity and had confirmed on man his discoveries made on dogs. Although he had never studied medicine, he knew more about diabetes than any physician. In November 1908 he delivered his Harvey Lecture on Metabolism in Diabetes, treating the subject from the viewpoint of the intermediary transformations of protein, fat and carbohydrate. Using eighty-three references he gave a condensed review of the important studies in this field, discussing particularly the severer manifestations of the disease.

In this lecture were laid down the principles which directed the laboratory and clinical studies of diabetes for the next two decades.

In the introduction to this discussion Lusk gave a quotation which coincided so closely with his own views that it may be considered as the guiding principle of his life work. He said, "Some may question the right of a laboratory man, a physiologist, to present to medical men a scientific discussion of a diseased condition. In defense I can only quote to you the stirring words of Magendie, written in Paris as long ago as 1836, as an introductory to his 'Elements of Physiology'; a copy of which I inherited from my father's library. Magendie said: 'In a few years physiology, which is already allied with the physical sciences, will not be able to advance one particle without their aid. Physiology will acquire the same rigor of method, the same precision of language and the same exactitude of result as characterize the physical sciences. Medicine, which is nothing more than the physiology of the sick man, will not delay to follow in the same direction and to reach the same dignity. Then all those false impressions which, as food for weakest minds, have so long disfigured medicine, will disappear.'"

In 1909 Lusk was invited to take the chair in physiology at the Cornell University Medical College on 28th Street and First Avenue, New York City, two blocks away from the Bellevue Medical College. These two groups of medical teachers had split apart in 1898, as a result of some internal quarrel. William Mecklenburg Polk, Lewis Stimpson, and W. Gilman Thompson had secured an endowment from Col. Oliver Hazard Payne and had persuaded Cornell University to establish a medical school in New York City. By 1909 Graham Lusk's reputation and influence in American medical education had placed him at the head of his field and Cornell offered him every facility he could desire, including funds for the construction and maintenance of an Atwater-Rosa-Benedict respiration calorimeter. At the same time Cornell decided to cut down the number of students and admit only those who had completed the requirements for a college degree. With many regrets at severing his

official connections with Bellevue Medical College, Lusk accepted the chair at Cornell, taking with him his laboratory associates, John R. Murlin and A. I. Ringer.

In 1906, while still at Bellevue Medical College, Lusk had published the first edition of his famous book, "The Elements of the Science of Nutrition," dedicated to "Carl von Voit, master and friend from whom the author received the inspiration of his life's work." This contained three hundred and twenty-six pages. The second edition in 1909, four hundred and two pages, third edition in 1917, six hundred and forty-one pages, fourth edition in 1928, eight hundred and forty-four pages. Inasmuch as the importance of the material discussed in these successive volumes remained fairly uniform, the increasing number of pages in twenty-two years gave a good index of the development of the science during the period of Lusk's greatest productivity.

The first edition was largely devoted to the work of the German investigators and in it particular attention was paid to the fundamental discoveries of Carl Voit and Max Rubner. Rubner was a voluminous writer but his style was so difficult that even the Germans shun the task of digesting his works. Lusk had the training and patience which enabled him to recalculate Rubner's tables and set forth the findings in clear perspective so that they became available not only to the English-speaking world, but also to many Germans who read the English more easily than the original. The book rapidly became the standard textbook on nutrition, and what is more important, a source book for all who wrote on the subject. The reason for this is well expressed in the preface—"The aim in the present book is to review the scientific substratum upon which rests the knowledge of nutrition both in health and disease. Throughout, no statement has been made without endeavoring to give the proof that it is true."

Lusk's methods in preparing the four editions of this book may well serve as an example for all writers on scientific subjects. Material was collected every year and almost every week. There was no ghost writing, not even the delegation to others of the task of looking up references. Lusk was able to afford a large, private library, probably one of the most complete

libraries of nutrition ever assembled. He subscribed to all journals bearing on the subject and kept the bound volumes on shelves within easy reach. He had a large collection of reprints and a fair number of historical works. He read carefully everything of importance in English, German, and French, and knew enough of the Italian and Scandinavian languages to get the gist of the matter. Every worthwhile report was recalculated and often he was able to find many important facts that had been overlooked by the writers who did not possess his background. Abstracts were written in longhand on large sheets of yellow paper and many tables were copied. When it came time to prepare the manuscript, the material was again written in longhand. Only when it had been completed was it turned over to his secretary. Every reference and every figure was checked. There were extraordinarily few errors in his publications.

All who visited or worked in his laboratory remember the picture of Graham Lusk at his large desk, surrounded by journals, writing on the large sheets of yellow paper. His deafness was a protection and he did not realize your presence until you came close to his desk. You hesitated to disturb him, but if the matter were urgent you could attract his attention by twirling the revolving bookcase at his right hand. He responded invariably with a smile and cheerfully gave his valuable time to your own immediate problem. Investigators from all parts of the world came to his desk and if they so desired, he went over their tables and manuscripts, figure by figure, word by word. Then he would give freely his best ideas for new experiments.

Of course there were many interruptions. If he were running a calorimeter experiment, he could snatch at most half an hour between weighings. He had many conferences with his laboratory associates and with his pupils. His lectures to the students were prepared just as carefully as his publications.

In 1906 at the time of the first edition there had been relatively little fundamental work in this country on metabolism in health and still less on metabolism in disease. The second edition in 1909 contained more American references but did

not differ greatly from its predecessor. It was this edition that was translated into German by Hess in 1910. The third edition, published in 1917 and reprinted several times and revised in 1923, is much larger. The pessimistic remarks about the lack of interest in metabolism in the clinic are replaced by a note of optimism in this preface.—“Laboratory methods to explain the inner processes in disease have been applied to hospital patients for thirty years or more in Germany. In the United States great advances have lately been accomplished in this direction. If such investigations are still further promoted by their discussion here, this writing will not have been in vain.”

In the same preface he thanks his laboratory associates, Murlin, Du Bois, Ringer and Gephart, and goes on to say: “It is furthermore a privilege to recognize the great influence which a personal acquaintance with such men as F. G. Benedict and S. R. Benedict, Cathcart, Chittenden, Cremer, Dakin, Folin, Halliburton, Hopkins, Kossel Levene Magnus-Levy, Lafayette Mendel, Friedrich von Müller, von Noorden, Rubner, E. Voit and Zuntz has had upon the conceptions of the subject of nutrition as set down in this book.” In the preface to the fourth edition he added A. V. Hill and A. E. Taylor to this list. Lusk visited the European laboratories frequently. Every physiologist who came to America visited Lusk.

The third edition was much larger than the second, giving more space to the history of the science and to food economics. The laboratory studies of American investigators began to occupy a relatively large share of the pages. Many gaps made evident in previous editions were filled by the work of Lusk and his pupils. This was only natural as the yearly preparation of lectures to students brought forward questions that could be answered by experiments in the laboratories adjoining the lecture hall. Charts from Lusk’s own careful work made many problems clear.

The fourth edition, published in 1928, is an expansion of the third, bringing the subject up to date. Lusk intended this to be the final edition. It still had a large sale ten years after its publication. No book has taken its place. It is the “Bible”

of the nutritionists. One finds throughout the literature its sentences and paragraphs slightly disguised by paraphrasing.

John R. Murlin was with Lusk as first assistant when he transferred to the new institution. In his review of Lusk's work (12) he writes:

"The move to Cornell University Medical College, only one block distant on First Avenue, in 1909 brought enlarged opportunities for prosecution of a program of research which had been forming in Professor Lusk's mind while he was revising his 'Science of Nutrition.' The second edition made its appearance coincidentally with this move to Cornell. During the summer of this year, while alterations for the laboratory at Cornell were in progress, Lusk went to Europe in order to put the finishing touches to his revision, and while there, on the recommendation of his first assistant who was working in the nutrition laboratory of F. G. Benedict at Boston, resolved upon the construction of a small respiration calorimeter of the Atwater-Rosa-Benedict type, suitable in size for study of the energy metabolism of dogs or of small children. What he desired most of all to investigate was the specific dynamic action of the amino acids. Dr. H. B. Williams, already a member of the department of physiology at Cornell, went to Boston and studied the construction of the calorimeter. J. A. Riche, trained by long experience in Benedict's laboratory, was engaged to operate the new calorimeter and assisted Williams in its construction, a large part of the mechanical work being done by these two men. Together with Professor Lusk they formed a research team of unusual ability, and the precision with which dependable results on this difficult problem were turned out was the result of clear comprehension of the physiological factors, combined with high technical skill. Williams, however, left soon to accept an appointment at the College of Physicians and Surgeons, Columbia University.

"The first work in the order of publication accomplished by the calorimeter was a paper by John Howland on the energy metabolism of sleeping children. Lusk had very generously set aside his own program to give Howland this opportunity, which had much to do with making him professor of pediatrics at Washington University and, a year later, at Hopkins. This work at the same time demonstrated the remarkable efficiency of the calorimeter which Williams had built."

When Lusk was abroad in 1909 I happened to be working with Borden S. Veeder under Brugsch in Kraus' clinic in

Berlin. Our problem was the estimation of the total metabolism in diabetes. One morning word passed through the laboratory, "Der Graham Lusk kommt heute," but, of course, his name was pronounced "Grah-ham Loosk." Veeder and I were busy with the large Pettenkofer-Voit chamber when Lusk came to our room accompanied by the respectful group of professors and dozents. Rather naïvely we started to demonstrate the apparatus, but Lusk in his charming manner let us know that he had been working in his own laboratory with the same type of respiration chamber. The next year in America, when Veeder and I were struggling to write our first scientific paper, Theodore Janeway sent us to Graham Lusk, who spent hours recalculating our tables and teaching us the principles of writing a scientific report. I have seen him give this same priceless help and stimulation to a hundred or more young men and I am sure that every one of them remembers his example when in turn younger men come to him. Lusk's influence spread geometrically, not arithmetically.

Perhaps I may be pardoned if I also use my own case as an example of Lusk's indirect as well as direct influence. In 1905 I had finished my third year in a New York Medical School, not Lusk's, untouched by Lusk's teachings and almost untouched by physiology. That summer Theodore C. Janeway, one of the instructors at Bellevue Medical College, offered at St. Luke's Hospital the first clinical clerkship ever given in New York. I am sure that he wished to experiment in teaching clinical medicine along the lines that he and Lusk had discussed so often. He certainly succeeded in implanting in us the idea of pathological physiology as a basis for the study of the patient. Again, early in 1909, when I was about to sail to France to study bacteriology, I happened to meet John Howland, the brilliant young practitioner of pediatrics, who had only a short time before come under Lusk's influence. He advised me very emphatically not to study bacteriology, but to go to Germany and learn something about metabolism, which was becoming the most important field in clinical medicine.

It takes a long time to build and test a respiration calorimeter and it was not until 1911 that Lusk's preliminary report was

made. In 1912 there began to appear in the *Journal of Biological Chemistry* the famous series of papers on animal calorimetry. Number XXXIX, the last with Lusk's name as an author, was published in 1930, but the series is being continued by his associate, W. H. Chambers. Throughout all these years the technique has been practically uniform. The same dogs have been studied several years. Each report fits in with the other reports as part of an orderly campaign of investigation. The calorimeter gives a wealth of detail, measuring in hourly periods the grams of carbohydrate, fat and protein metabolized, the total heat production, heat storage, heat of vaporization. Analyses of urine and blood throw light on the intermediary processes. Never in metabolism have there been such complete studies.

First the effects of the ingestion of meat in large quantities were determined, then the various amino acids, carbohydrates, fats, mixtures of these. Later the influence of the glands of internal secretion was investigated and always the baffling problem of the cause of the specific dynamic action of foods, the stimulation of metabolism that follows the digestion of all foods but especially protein and carbohydrate. Later there were many experiments with dogs working in the calorimeter on a treadmill. Often the Lusk method of phlorhization was used as the key to the maximal production of glucose from protein and the various amino acids.\*

Meanwhile Lusk had not forgotten his interest in clinical medicine. P. A. Shaffer, the biochemist, and Warren Coleman, the clinician, both teachers at the Cornell Medical College, had

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\* The first paper of the Animal Calorimetry series, by John Howland, was published in 1911 in the *Zeitschr. Physiol. Chem.*, 74, 1-12. During Lusk's life time there were 1432 calorimeter experiments. Between the time of his death and July 1939, 693 additional observations were made in the same calorimeter. The following members of Dr. Lusk's department of volunteer workers whose names do not appear in his bibliography, contributed to the Animal Calorimetry series, published in the *Journal of Biological Chemistry*, 1912-1932:

Gertrude Fisher, Mary B. Wishart, F. A. Csonka, Einar Langfeldt, Sophia A. Taistra, M. Ringer, R. Weiss, M. Wierzuchowski, S. M. Ling, S. S. Waddell, J. A. Mandel, A. T. Milhorat, H. Ellis, C. Wilson, Folke Nord, O. H. Gaebler, Margaret A. Kennard, H. Pollock.

The calorimeter technician was James Evenden. All calculations were checked by Lusk's indispensable secretary, Miss Phillippena Schaub who was devoted to his interests during the whole of his period at Cornell.

made important studies in the metabolism in typhoid fever. In 1911 Lusk secured funds for a study in the respiratory metabolism of typhoid, and E. F. Du Bois was given an appointment at Bellevue so that he could assist Coleman. After some preliminary work on the absorption of food in typhoid fever and the respiratory metabolism, using a Benedict unit respiration apparatus, Lusk felt justified in asking for the transfer to Bellevue Hospital of the Russell Sage Institute of Pathology. This had been established in 1903 by a gift from Mrs. Russell Sage, and had been devoted to studies in pathology at the City Hospital on Blackwell's Island. Unfortunately there had been difficulty with the city politicians and work had to be discontinued. Lusk was one of the directors in 1912 and the fellow members of his board gladly adopted his suggestion of building a respiration calorimeter and establishing a metabolism ward in Bellevue Hospital, adjacent to the teaching wards of Cornell Medical College. In 1913 Lusk was appointed Scientific Director, Du Bois, Medical Director. Since that time the yearly income of twelve to sixteen thousand dollars has been devoted to the study of metabolism in disease, one of Lusk's chief interests since 1899. The calorimeter for patients was built in 1912 by Riche and Soderstrom and on March 13, 1913 Lusk himself was the first experimental subject. His metabolism, supposedly basal, was probably too high in his excitement over the realization of one of the dreams of his life. The protocol of the experiment (5) records dryly, "G. L. physiologist, large frame, slightly adipose. Has taken but little exercise during the last few years. Health good. No recent illnesses. Physical examination negative." His height was 175.5 cm. (5 ft. 9 in.), his weight 78.42 Kg. (175 lbs.).

Lusk in 1915 wrote the first paper of the clinical calorimetry series describing "a respiration calorimeter for the study of disease," and took an active part in guiding all of the papers up to No. 50, published the year of his death. As in the case of the animal calorimetry series, the papers are continuing along the lines he so carefully planned. In the experiments on man the technique was almost the same as in the dog experiments. Often the two calorimeters would be working on the same problem.

After every experiment in the hospital the record sheets would be taken to Lusk's desk and the findings critically discussed. Almost every afternoon Du Bois walked home with Lusk, talking over plans for more work, trying to absorb Lusk's views on metabolism, medical education and life in general.

The studies of the Sage staff were devoted first to the basal metabolism of normal controls, the surface area of the body, typhoid fever, and then diabetes, guided by a classical article of Lusk's on the diabetic respiratory quotient. Later many other diseases were studied with the aid of visiting clinicians of New York and other cities.\*

In addition to the animal and clinical calorimetry series, Graham Lusk published many papers on intermediary metabolism. He dealt with the amino acids, glycogen, alcohol, phlorhizin-glycosuria, the specific dynamic action, diabetes, acidosis, undernutrition and the endocrines. In 1910 he was particularly interested in the removal of the glycogen stores in the body by means of shivering. He kept his plan secret, and one morning the laboratory staff was horrified to see him submerged in a tub of water full of cakes of ice, while an assistant was measuring his respiratory metabolism.

Throughout his life Lusk was striving continually for the betterment of medical education and his addresses and writings on this subject were vigorous. He was ahead of his time, and he knew it, and he was sorry that he had to tread on the toes of many of his personal friends, but he never faltered. At first, he was whole-heartedly in favor of the so-called "full-time medicine." Later he modified his views when he felt that the pendulum had swung too far.

During the World War his attention was naturally drawn to

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\* Collaborators in clinical calorimetry series. Papers 1 to 50, 1915 to 1932. Clinical Calorimetry papers, 1 through 32, published in the *Arch. Int. Med.*, 1915 to 1922; papers 33 through 48, published in the *Jour. Biol. Chem.*, 1923 to 1932; papers 49 through 53, published in the *Jour. Nut.*, 1938. J. A. Riche, G. F. Soderstrom, F. C. Gephart, E. F. Du Bois, W. Coleman, Margaret Sawyer, R. H. Stone, D. Du Bois, A. L. Meyer, F. W. Peabody, F. M. Allen, J. C. Aub, J. H. Means, J. B. Murphy, D. P. Barr, W. S. McCann, R. L. Cecil, H. B. Richardson, E. H. Mason, W. S. Ladd, A. M. Michaelis, S. Z. Levine, W. S. McClellan, A. Biasotti, R. R. Hannon, H. J. Spencer, E. A. Falk, V. Toscani, V. R. Rupp.

food economics, and he wrote both scientific and popular articles on food in war time, paying particular attention to the caloric needs of children. When the United States entered the war, he offered his services to the Government, and was sent to England and France as one of our two food experts. In an article on R. H. Chittenden (1929), he writes:

"In the winter of 1918 Chittenden and I went to Europe as members of the Interallied Scientific Food Commission under instructions from our Government to reduce the food requisitions upon the United States to a minimum. The Food Committee of the Royal Society had adopted 3000 utilizable calories per day as the requirement of an average man doing an average day's work, and at the Paris meeting of the Interallied Commission their representatives were inflexible in holding to this position. Before one of the meetings, while walking over the Pont Royal, which took us to the left bank of the Seine, Chittenden said to me, 'Lusk, we are here to aid these suffering peoples to the maximum of our power.' A few minutes later he said before the startled commission, 'If you will not hear us we might as well go home.' This led to the unanimous adoption of a modification of statement that read: 'It was agreed that in case this ration could not be provided, a reduction of not more than 10 per cent could be borne for some time without injury to health.'"

Lusk enjoyed his stay in the war zone and his association with the nutritionists of the allied countries. One of his chief contributions was a chart of the caloric requirements of the different trade and age groups, and it was he more than anyone else who secured for adolescent children a food allowance equal to that of adults. Although everyone knows that children eat more than their parents, the older textbooks had fixed their allowance on body weight rather than on the needs of the developing organism. There were millions of children in France, England, and especially Belgium, who were indebted to Lusk for the privilege of normal growth.

With advancing years the history of nutrition became his chief hobby, almost his only hobby. He tracked down Lavoisier's original respiration mask, and wrote articles on this great founder of the science of metabolism. He wrote on Liebig, Voit, Rubner and others. Delightful quotations were inserted at the head of each chapter of the fourth edition of the "Science of Nutrition."

At the time of his death he had just completed a history of nutrition in the *Clio Medica* Series.

It must not be thought that Graham Lusk was merely a specialist in nutrition with a particular interest in metabolism in disease. He was above all a great physiologist and a great professor of physiology. His department was organized along broad lines. While his own lectures were confined to the physiology of digestion and metabolism, he was ably supported by associates who covered the rest of the field, Murlin, H. B. Williams, Carl Wiggers, D. R. Edwards, McKeen Cattell and others. He sent these men to work in other laboratories in this country and Europe. He took a keen interest in the large volume of important work in circulation carried out in his laboratory. There was a constant succession of research men sent to his laboratory from the United States and Europe. Most, but not all, came to work with Lusk himself. Perhaps the thing that gave him most pleasure was the large number of his own students who participated in the research of his department.

Graham Lusk was above the average height, as a young man quite thin, as an older man slightly overweight. His hair was brown, almost red, his eyes piercing with a humorous twinkle. He was quick in his movements, gesticulating effectively, full of life except when absorbed in reading or writing. In conversation he gave you his full attention. His deafness was not a serious handicap. He could hear you well if you raised your voice moderately. He loved conversation, talked easily and always entertainingly. An electrical appliance permitted him to follow lectures if he sat in the front row. It was a little difficult for him to hear all that was said at committee meetings or in general conversation, and his friends realized that some of his outbursts were due to a misunderstanding of statements. He could not always appreciate the sound of his own voice or laughter and when excited it would become high-pitched, vibrant and a little harsh. Ordinarily his voice was very pleasant; his laughter spontaneous and hearty. He laughed often for his spirit was happy. He was never depressed. He never really lost his temper but every few months he came near doing so when he encountered some injustice or heard or read a state-

ment which ran counter to his pet beliefs. His natural impulse was to take immediate action and the members of his staff figuratively tried to hang on to his coat tails until he cooled down.

Lusk was most easily excited by the problems of medical education and the controversial subjects of metabolism in diabetes, the derivation of carbohydrate from protein and fat, the specific dynamic action and the relationship of basal metabolism to the surface area of the body. He had great reverence for the ideas of Voit and Rubner and was inclined to take up cudgels when their doctrines were assailed although he realized fully that these two men were not infallible in all their findings. He was rather inclined to overestimate the importance of the work of his associates and pupils and often rushed impetuously to their defense. He never made his attacks personal. He seldom questioned technique but confined his criticism to theoretical interpretations. One such controversy with F. G. Benedict and E. P. Joslin dealt with the basal heat production in diabetes, which Lusk believed to be at about the normal level. Joslin in his article on Graham Lusk (23) has described this delightfully:

"Hot debates followed about the metabolism in diabetes. Was it increased or decreased? I even wrote a book about it and my experiments lasted so many years that the first ones, in the Naunyn Era of liberal diets showed what we in Boston interpreted as an increased metabolism and the second series, performed subsequently in the era of undernutrition, a relatively decreased metabolism. Neither laboratory quite hauled down its flag but listened to what Graham Lusk wrote when he disagreed with the Carnegie Nutrition Laboratory—'Whatever of criticism may be found in the following lines, it is to be borne in mind that there was never any question of the absolute accuracy of all this work; the criticism regards only the interpretation.' Don't you see here that the fourth drawer in the Lusk Bureau of Standards was one of searching, constructive, but always just and friendly criticism."

Another longstanding debate was with F. G. Benedict who was sharply opposed to the doctrine of Rubner that the basal metabolism of different animals depended on the surface area. This discussion waxed and waned for twenty years. Lusk had a great admiration for Benedict who was one of the four authors

most often quoted in the "Science of Nutrition," but he simply could not stand by and see his pet theory attacked. Even more serious was the doctrine of Macleod and his school in England and Canada that glucose could be formed from the fatty acids. Lusk and his pupils threw all their energies into combatting this conception. With all of the sharp differences of opinion there was no personal bitterness and Lusk retained friendship and respect. He always stopped short of the polemic.

It was a privilege to see him in action in discussion at physiological meetings. He was generous in his praise of good work, firm in his criticism of what he thought was wrong. As a rule he knew the literature better than the speaker. When Lusk and Meltzer were attending meetings in New York, the standards of papers were high, for it was well known that poor work would be greeted by plain words.

Lusk was as kind as possible with young men but fearless and almost relentless in his attacks on big game such as university faculties or government bureaus or large foundations. One of the best examples of his method is a letter to "Science" written in 1915. The following condensation and slight rearrangement is taken from Swift's "Influence of Graham Lusk on Medical Education." (25)

"It is impossible in any faculty to approach this subject without hurting the feelings of true and honorable men, men who deserve well of their country and who are not to blame for the present situation brought about by an altered trend of educational thought. It is, therefore, extremely difficult to speak of these matters without seeming to be both unkind and unjust. On the other hand, if no word is spoken, blame for cowardice is incurred. . . . The truth of the matter is that, as a country, we have produced few men in medical science. This is frankly because the teaching of medicine has not been in accordance with modern science. The staff of the medical department should consist of men, themselves devoted to medical science, capable of carrying it on, brought up in the air of it and blessed by the enthusiasm of it. Such men should be produced under the leadership of the professor of medicine. . . . Other remedies are only temporary palliatives. The medical school owes a duty to the public. Personal ambition, even though unconsciously exercised, should not be allowed to frustrate the fulfilment of the duty to the community which the college lives to serve. The schools are brought

face to face with the question whether their policy will be to advance along modern lines or stand still yet a little while."

In this and subsequent writings on the subject Lusk attacked the entrenched positions and precious heritages of many of his best friends in clinical medicine. It was he more than anyone else, with the possible exception of Welch and Abraham Flexner, who brought about the reform in the clinical departments of our best medical colleges.

Lusk spoke well at dinners and public meetings. He spent many days in the preparation of his material and always brought out something of significance. His delivery was clear and vigorous; his spirit and humor were contagious. The audience listened attentively for he had the gift of "letting himself go," speaking from the bottom of his heart. There was no "oratory" but something much more effective.

His lectures to the students were prepared and delivered with the same care. William S. McCann, one of his most distinguished pupils, who took the course in 1913, has described the lecture in his glowing tribute, "The Influence of Graham Lusk upon His Students." (22)

"The morning period in the physiological laboratory always began with a lecture. The Professor gave part of the lectures on digestion and all of the lectures on nutrition. His two assistants at that time, John R. Murlin and Carl Wiggers, made with him a trio that has rarely been surpassed in strength. In his lectures the Professor followed closely the text of his famous book which we kept open in front of us as a sort of syllabus. My old copy is abundantly underlined and annotated. Even today as I look at it the whole scene comes vividly to mind and I can hear the rising and falling cadence of his voice, now scarcely audible and again pouring forth with the vehemence of strong feeling.

Someone has said that if teaching consisted only of the imparting of factual knowledge then Universities might well have ceased to exist with the invention of printing. Graham Lusk's lectures provided superb examples of the reasons why the living voice has not given way to the printing press. There on the page before us were the facts tersely marshalled in logical sequence. Through our ears came interpolations of personal anecdotes which made those facts into a sort of aura of the living beings who discovered them. Lavoisier was our daily companion, as

were Carl Voit and Max Rubner, while Magendie and Claude Bernard appeared never less than once a week. . . . The more usual anecdote raised its subject to the heroic proportions of a legendary figure. The mind of Graham Lusk was a special Valhalla to which were conveyed dead heroes of the test tube or stethoscope: in it places were reserved for living heroes, for no man was more generous in appreciation of his contemporaries than was Lusk. In this legendary world Carl Voit was Woden, von Mueller was Balder, and Jove himself was not mightier than Rubner.

So in these lectures the prosy pages of his book became alive, and peopled both with common men and heroes. Just as the youths of ancient Greece, or our Teutonic ancestors, were inspired by the epic performances of their heroes, so we were inspired.

As we went about our own tasks in the laboratory we could see the Professor at his. The humblest task in an experiment was not beneath the professorial dignity. Did we not see him feed his dogs with his own hands? If a dog were to be catheterized in one of his own experiments the Professor did it himself. In his calorimeter room he worked as a member of the team making readings and weighings, checking the observations and calculations of his humbler subordinates, *and being checked by them*. A complete communism existed in that room for the duration of an experiment. One might see Soderstrom or 'Jimmie' Evenden vehemently arguing with the Professor about a technical matter, while he would stand in his characteristic judicial attitude with his chin encircled by his hand, or with his index finger beside his nose, gravely listening and sometimes nodding his head."

Lusk's scientific contributions were so highly technical that it is inadvisable in a brief biography to discuss them in detail. They have been well summarized by Murlin in his article in the *Journal of Nutrition* (12). Lusk in the 1928 edition of the "Science of Nutrition" gives eighty-five references to his own work but quotes Rubner twice as often. In a review, "Fifty Years of Progress in the Chemistry of Physiology and Nutrition in the United States 1876-1926," he mentions briefly his dextrose to nitrogen ration of 3.65 in the fasting and meat-fed dog under the influence of phlorhizin and the same ratio that he and Mandel found in a totally diabetic man. All the rest of his work he credits to his collaborators. Murlin has well described his painstaking search for the cause of the specific

dynamic action, that rise in metabolism which follows the ingestion of food. Proteins, fats, carbohydrates and products of the intermediary metabolism were studied in great detail year after year. At first he held Rubner's explanation that the increment results from the metabolism of the intermediary products themselves. Time changed his views as evidence was presented but he was never satisfied and finally returned to Rubner's theory. His experiments have always been source material for those who make calculations in this field and they will remain as standards until some new method of experimentation is discovered. Lusk never solved this problem to his own satisfaction although he was the man best equipped to find the answer to this riddle. His knowledge of the literature was enormous, his laboratory equipment the best in the world, his technique exact and constantly checked. Experiments were planned with the greatest care and few were wasted. Each one was directed to throw light on some specific question. He used relatively few experiments, basing his conclusions on a small number of reliable tests rather than on a mass of material treated statistically.

Graham Lusk with his background of scientific attainment and his forceful personality naturally assumed a position of leadership in the scientific circles of this country. He was one of the founders of the Society for Experimental Biology and Medicine (6, 7), and in 1914-15 its president. Close to his heart was the Harvey Society of which he was the founder, first president and the only life member of its council. His address at the twenty-fifth anniversary on August 15, 1930, records medical history:

"The story of the birth of the Harvey Society is a simple one. I was dining in the old Lusk home at 47 East 34 Street and sat next to Mrs. Anna Bowman Dodd. You will remember that it was she who wrote many years ago 'Three Normandy Inns.' The greater part of her life she lived in France; in Paris in the winter, and in a beautiful home at Honfleur on the Normandy coast in the summer. She has recently passed away at the age of about eighty. It gave her pleasure to the end to be told that she was the real founder of the Harvey Society. At the dinner to which I refer she said that during the winter she had attended a course of splendid lectures at the Sorbonne upon the subject of Roman law expounded by a brilliant Frenchman. It occurred

to me that if an educated American woman past middle life could be thrilled by lectures on Roman law, there must be physicians in New York who would be interested in hearing lectures on scientific subjects as expounded by scientific workers themselves. There was only one man with whom to go into conference on this subject and that was Dr. Samuel J. Meltzer. Meltzer had already used the library of my home at 11 (now 9 and rebuilt) East 74th Street, for in it a few years before, he had founded the Society for Experimental Biology and Medicine, sometimes for the sake of abbreviation affectionately known as 'The Meltzer Verein.' This was to be a society of scientific *workers*, and is today a notable feature of the Academy of Medicine. In response to a telephone call Meltzer came to see me immediately and, sitting together on a sofa, I outlined my plan. He said the idea was impossible; New York was a city devoid of scientific interests. The Academy of Medicine was not a scientific body and had no interest in scientific medicine. No one would come to the meetings and it would be futile to start such a movement.

A few days after this Meltzer called me on the telephone and said, 'You must call that meeting at your home.' I replied, 'But, Dr. Meltzer, you said the plan was impossible.' 'Ah, but I have changed my mind.'

So it came about that there met at my home on the anniversary of Harvey's birth, April 1, 1905, the following group of men: Meltzer, W. H. Park, E. K. Dunham, Ewing, Lee, Herter, Flexner, Wallace, T. C. Janeway, Levene, Opie, Abel of Baltimore, and Lusk. I outlined the plan. Everyone objected, using the same arguments which Meltzer now convincingly answered. His final words were, 'Never mind if no one comes except ourselves. We will wear our dress clothes, sit in the front row and show the speaker that we appreciate him.' . . . The society was made up of a group of young men. I remember giving a dinner of thirty to Professor Max Rubner, of Berlin, nearly twenty years ago, and he, surveying the table, said to me, 'You have no old men in America.' As far as our scientific group was concerned, this was then true. Scientific medicine in New York stood at the beginning of time."

The Harvey Society under Lusk's guidance was a great success from the very beginning. It is still simple, a series of seven or eight lectures every year, a published volume of these lectures. An invitation to lecture is one of the great honors bestowed in our country.

Lusk was a leading spirit in the Federation of Biological Societies. He was instrumental in founding the Society of

Biological Chemistry and was its president in 1914. He was a prominent member of the American Physiological Society. He was elected to the National Academy of Science in 1915 and took great interest in the meetings. He was in his element at the International Physiological Congresses held every three years as he had many friends among the physiologists of Europe. He had a great deal to do with the marked success of the thirteenth congress held in Boston in August 1929. After the members left Boston and made their way to New York he entertained 475 of these at his lovely country place at Syosset on Long Island. In 1928 he was one of the founders of the American Institute of Nutrition. He served on the editorial committees of various journals but never assumed the time-consuming duties of a chief-editorship. He belonged to so many societies and received so many honorary degrees that they have to be listed at the end of this article.

Graham Lusk was modest in regard to his own attainments but he knew perfectly well the strength of his position in the community. He had the security of family background, independent means, hosts of friends and pupils, a prominent academic position, high reputation as a teacher and scientist. In the twenty-one years that I knew him he never raised a finger for his own advancement, in none of his many battles did he ever ask a thing for himself. It is not surprising that eventually he secured almost all his objectives. He had an open mind and was ready to yield a point against which he had contended for many years once adequate proof that he was mistaken came to hand. His life was singularly devoted to science and he indulged in relatively few recreations except travel and mountain climbing. He enjoyed good food and good wine but always in moderation. When conducting a calorimeter experiment he did not have time for lunch. He walked a great deal in New York City and in the country. On Sundays he attended the Presbyterian Church under preachers whose breadth of religion was compatible with scientific thought. He seldom spoke of religion himself but consistently led a Christian life, loving and helping his fellow men. Politics interested him spasmodically and at times he was quite vehement on the subject but took no active

part in campaigns. Usually he was a Republican. Through his work in the Interallied Food Commission he developed great admiration for Herbert Hoover.

Lusk's outstanding characteristic was his capacity for friendship with men of all ages. His old friend George B. Wallace (9) has said of him: "He was of a genuinely friendly nature and had a great attraction for young men. Those men whom he accepted as friends he believed in implicitly and without reservation." He knew almost all the prominent physiologists of the world and they were his guests when they visited New York. In his beautiful tribute to the life and work of Max Rubner, delivered two months after Rubner's death and one month before his own death, he says:

"Great men are rare. They are worth knowing. They give impulse and stimulus to lesser men. They make the world more worthwhile for others to live in because of their presence in it. Max Rubner was the greatest man I ever knew.

The first time I met Rubner I called on him at his laboratory in Berlin . . . but I never really knew him until he visited me in the United States when he came over to attend the Fifteenth International Congress of Hygiene and Demography, which was held in Washington in 1912. He first visited me in my Adirondack camp, and we gave him a tent in which to sleep. . . : I gave a dinner in his honor at the University Club in New York. Nuttall, professor of hygiene at Cambridge, England, who had been his pupil, was seated on my left. He said to him, 'I expected to feel like a stranger in America, but I feel nothing of the sort, and when I talk to Lusk it is as though I were back in Munich.'

The last visit we made to Rubner was at his home at Pinszwang, a village in the Tyrol just over the Austrian border, a short distance from Füssen, and we thus fulfilled a promise made to him eighteen years before. He was in fine spirits and brought out some excellent wine of which he was very fond. He went with us in the afternoon to Füssen. . . . I can still see his distinguished figure standing at the end of the stone bridge at Füssen, as he raised his hat and waved it in farewell."

One of his laboratory assistants has written (16):

"Perhaps the chief service of Graham Lusk was his constant readiness to help younger men. There are hundreds of us who have gone to him with our problems, and we have always received his aid and inspiration. Not only the men who worked in his own laboratory but those from far distant parts of this

country and Europe are indebted to him for much of the best parts of their publications. Unless he himself had taken an active share in the conduct of an experiment he would never attach his name to a paper. Whenever he published with younger men his name appeared last and there were no heart-burnings in his laboratory over that coveted first position on the title page which is of value only to those men who would otherwise remain insignificant. On rare occasions, when he felt that a young man was insincere or totally unprepared for his task he would act firmly and promptly for the good of the scientific world. For all others he was full of encouragement and appreciation, and he cherished the enthusiasm of youth because he himself retained it to the last day of his life."

No one realized more fully the hazard which advancing age brings to a scientific reputation. In an address to students delivered in 1930, he says:

"There is one picture which I would like to present to you which belongs to the history of human progress, as illustrated by the criticism by Berzelius of Liebig and his reply; the criticism of Liebig upon Voit and its repercussion; with a brief reference to the opinion of Voit upon the younger generation. The story is the same, the inability of a man in the sixties to understand a young man or to suffer criticism from him."

He tells the story of Berzelius and Liebig and continues:

"An attack on Voit's work by Liebig came about 1870 when Liebig was sixty-seven and Voit was thirty-eight years old. Voit was stung to reply. Perhaps, by recalling the story, it may be instructive to bring out this historical cleavage between the thoughts of an older from those of a younger generation. It needs no emphasis on my part to impress you with the fact that if some of you of the younger generation are not better trained than the masters of an older generation, then further scientific advance will not be possible. And I here recall the words of one of our great American medical prophets, Samuel J. Meltzer:

"I shall continue to work as long as I live. There are only two things which could stop me. If any one said to me, 'Meltzer, your work is no longer good,' then I would stop. Or if anyone said to me, 'Meltzer, you can no longer understand a young man,' then I would stop also."

Next comes the story of Liebig and Voit:

"A generation later, when Voit was about sixty, I heard him say that there were no good young men of forty in Germany

at a time when Rubner, Kossel, and Hofmeister were that age. One of these men has written me that modern workers deal only with *Kleinigkeiten* (small things). Historically speaking, there is an age disability in his judgment of the young when a man is over sixty.

"In three successive generations in which the younger man was forty and the older man sixty there was a lack of appreciation of the real ability of the younger man. Berzelius criticised Liebig; Liebig criticised Voit; Voit did not fully appreciate Rubner."

Finally, applying the lesson to himself, Voit's pupil, he speaks of his own criticism of Macleod's book, "The Fuel of Life." He concludes:

"But such a book as that of Macleod has a high value, since it constitutes a challenge to do productive work. Macleod is a man of the highest personal character, of high scientific achievement, but, as we have learned today, this is not protection against the destruction of theories evolved through misplaced reliance upon erroneous or incomplete experimental evidence. The words of old Voit come to me, 'It makes no difference *who* is right so long as the truth be found.' I realize that, from the historical standpoint, I am treading perilous ground in daring to criticise the work of one younger than I am."

Again in 1932, shortly before his death, he said to Lafayette B. Mendel, "I can well recall the words of criticism that I have often uttered somewhat violently, when I was in my thirties and forties, about persons whom I regarded as antiquated and reactionary medical teachers of my present age. I shall therefore endeavor to avert a similar fate for myself at the hands of the present-day youngsters by retiring." He had reached the age of sixty-five which was the retiring age in his university. It so happened that in this very year the Cornell Medical College, after a long period of striving, had completed its magnificent new building in association with New York Hospital, made possible by the legacy from Payne Whitney. Lusk had taken an active part in the planning of this affiliation and it represented one of the greatest accomplishments of his life's work in medical education. As the old building on 28th Street was abandoned he appeared as one lost without the prospect of teaching and research. He felt so strongly about it that he would not speak of

his future or accept the offer of rooms where he could work. Can anything be harder for a man who is still active in mind and body than to terminate abruptly a distinguished career? Lusk's heart and soul were in his work. There was no one in his field with his ability or knowledge or breadth of experience. Still he believed that the retirement system was necessary and wise in spite of its cruelty.

Within a few weeks of his last duties as professor of physiology, while still in excellent general health, it became evident that he would have to submit to a serious operation. Complications developed and he died on July 18, 1932. His life work had been completed. He had won his main objective in his fight for medical education. He had made scientific discoveries of permanent value. One of his greatest contributions was his textbook. Of this A. J. Carlson (26) has said: "If there be any one book having had a wider and more penetrating influence on medical research in this country than Lusk on the Science of Nutrition, I do not know it. If there be, to date, by the pen of any other one man in any language, a better discussion of the whole scope of the science of nutrition, I have not seen it."

Even greater was the example of his own life and character. It has made a profound impression on his pupils and associates. Thus Murlin when he introduced Lusk at a public lecture spoke as one of a large multitude when he said: "For fourteen years your chairman was associated with the speaker as pupil, assistant, and colleague and he now states from the heart that he has never known a man who combined in so happy a way the solid merits of the scientist with all that is finest of courtesy, kindness and culture in a true American gentleman."

TITLES AND HONORS

Columbia University, School of Mines, Ph.B., 1887  
University of Munich, Ph.D., 1891  
Yale University, Honorary A.M., 1897  
Yale University, L.L.D., 1908  
University of Glasgow, L.L.D., 1923  
University of Munich, Honorary M.D., 1927

MEMBERSHIPS

Fellow, Royal Society of Edinburgh  
Corresponding Member, Imperial Society of Physicians, Vienna  
Member, National Academy of Sciences  
Representative of United States, Interallied Scientific Food Commission,  
1918  
Corresponding Fellow, National Academy of Saxony, Leipzig  
Member, Deutsche Akademie der Naturforscher zu Halle  
Associate Member, Société Royale des Sciences Médicale et Naturelles,  
Brussels  
Associate Member, Société de Biologie, Paris  
Honorary Member, Des Moines Academy of Medicine  
Honorary Member, Physiological Society of Great Britain  
Honorary Member, Physiologische Gesellschaft of Berlin  
Corresponding Member, Preussische Akademie der Wissenschaften  
Foreign Member, Royal Society of London  
Member, American Physiological Society  
Member, Harvey Society  
Member, American Society of Biological Chemistry  
Member, Society for Experimental Biology and Medicine

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Amer. Jour. Med. Sci.—American Journal of the Medical Sciences  
 Amer. Jour. Physiol.—American Journal of Physiology  
 Amer. Jour. Pub. Health—American Journal of Public Health  
 Amer. Med.—American Medicine  
 Amer. Mus. Jour.—American Museum Journal  
 Amer. Textbook Physiol.—American Textbook of Physiology  
 Ann. Med. History—Annals of Medical History  
 Arch. Int. Med.—Archives of Internal Medicine  
 Biochem. Zeitschr.—Biochemische Zeitschrift  
 Biol. Med.—Biology and Medicine  
 Boston Med. & Surg. Jour.—Boston Medical and Surgical Journal  
 Bull. N. Y. Acad. Med.—Bulletin of the New York Academy of Medicine  
 Bull. Soc. chim. biol.—Bulletin de la Société de Chimie Biologique  
 Canada Med. Assn. Jour.—Canadian Medical Association Journal  
 C. R. Acad. Sci.—Comptes rendus hebdomadaires des séances de l'Académie des Sciences  
 Deutsches Archiv. klin. Med.—Deutsches Archiv. für klinische Medizin  
 Ergeb. Physiol.—Ergebnisse der Physiologie biologischen Chemie und experimentellen Pharmakologie  
 Gaz. d. hop.—Gazzetta des hôpitaux civils et militaires  
 Ind. Eng. Chem.—Industrial and Engineering Chemist  
 Johns Hopkins Hosp. Bull.—Johns Hopkins Hospital Bulletin  
 Jour. Amer. Chem. Soc.—Journal of the American Chemical Society  
 Jour. Amer. Dietet. Assn.—Journal of the American Dietetic Association  
 Jour. Amer. Med. Assn.—Journal of the American Medical Association  
 Jour. Biol. Chem.—Journal of Biological Chemistry  
 Jour. Mus. Nat. Hist.—Journal, American Museum of Natural History  
 Jour. Nutr.—Journal of Nutrition  
 Jour. Pharmacol. Exp. Therap.—Journal of Pharmacology and Experimental Therapeutics  
 Jour. Physiol.—Journal of Physiology  
 Jour. Wash. Acad. Sci.—Journal of the Washington Academy of Sciences  
 Med. Clin. N. Amer.—Medical Clinic of North America  
 Med. Rec.—Medical Record  
 München med. Wchnschr.—Münchener medizinische Wochenschrift  
 New England Jour. Med.—New England Journal of Medicine  
 N. Y. Med. Jour.—New York Medical Journal  
 N. Y. State Jour. Med.—New York State Journal of Medicine  
 N. Y. Univ. Bull. Med. Sci.—New York University Bulletin of Medical Science  
 Physiol. Rev.—Physiological Review  
 Pop. Sci. Mo.—Popular Science Monthly  
 Proc. Amer. Physiol. Soc.—Proceedings, American Physiological Society

- Proc. Inst. Med., Chicago—Proceedings of the Institute of Medicine of Chicago  
 Proc. Nat. Acad. Sci.—Proceedings of the National Academy of Sciences  
 Proc. Soc. Exp. Biol. Med.—Proceedings of the Society for Experimental Biology and Medicine  
 Proc. XVII Intern. Cong. Med.—Proceedings, XVII International Congress of Medicine  
 Sci. Mo.—Scientific Monthly  
 Skan. Arch. Physiol.—Skandinavisches Archiv für Physiologie  
 Trans. Coll. Phy., Phila.—Transactions, College of Physicians, Philadelphia  
 Trans. XV Intern. Cong. Hyg. & Demog.—Transactions, XV International Congress of Hygiene and Demography  
 Yale Jour. Biol. and Med.—Yale Journal of Biology and Medicine  
 Yale Med. Jour.—Yale Medical Journal  
 Zentralb. Physiol.—Zentralblatt für Physiologie  
 Zeitschr. Biol.—Zeitschrift für Biologie  
 Zeitschr. physiol. chem. Hoppe-Seyler's—Zeitschrift für physiologische Chemie

# BIBLIOGRAPHY

1890

- Ueber den Einfluss der Kohlehydrate auf den Eiweisszerfall. *Zeitschr. Biol.*, 27, 459-81.

1891

- Influence of the carbohydrates on proteid catabolism with special reference to diabetes. *N. Y. Med. Jour.*, 54, 628-30.

1892

- Formation of glycogen after taking different varieties of sugar. *N. Y. Med. Jour.*, 56, 197-99.

1896

- Metabolism in diabetes. *Yale Med. Jour.*, 2, 187-92.  
 The chemistry of the animal body. *Amer. Textbook Physiol.*, Howell, Phila., 943-1021.

1896-97

- Recent discoveries concerning the physiological behaviour of different sugars. *Yale Med. Jour.*, 3, 199-204.

1898

- Ueber Phlorhizin-Diabetes und über das Verhalten desselben bei Zufuhr verschiedener Zuckerarten und von Leim. (With E. L. Ulricson, E. A. Newburgh, and I. W. Heller.) *Zeitschr. Biol.*, 36, 82-114.  
 The gastric inversion of cane-sugar by hydrochloric acid. (With S. J. Ferris.) *Amer. Jour. Physiol.*, 1, 277-281.  
 Phlorhizin diabetes in dogs. (With F. H. Reilly and F. W. Nolan.) *Amer. Jour. Physiol.*, 1, 395-410.

The chemistry of the proteid molecule. Discussed from the standpoint of animal metabolism. *Yale Med. Jour.*, 4, 427-32.

On metabolism in fatty degeneration. *Proc. Amer. Physiol. Soc.*, p. v.

1899

On metabolism during a combination of phosphorus poisoning and phlorhizin diabetes. (With W. E. Ray and T. S. McDermott.) *Amer. Jour. Physiol.*, 3, 139-155.

On the maximum production of hippuric acid in rabbits. (With W. H. Parker.) *Amer. Jour. Physiol.*, 3, 472-84.

1901

Metabolism in diabetes. (With W. L. Foster.) *N. Y. Univ. Bull. Med. Sci.*, 1, 147-150.

Ueber Phlorhizin Diabetes. *Zeitschr. Biol.*, 42, 31-44.

1902

The common use of alcohol as a beverage. Read at the annual meeting of the Massachusetts Medical Society, June 11, 1902. 10 pp.

On the growth of suckling pigs fed on a diet of skimmed cow's milk. *N. Y. Univ. Bull. Med. Sci.*, 2, 188-192.

1903

On the formation of dextrose in metabolism from the end-products of a pancreatic digest of meat. (With P. G. Stiles.) *Amer. Jour. Physiol.*, 9, 380-385.

Respiration experiments in phlorhizin diabetes. (With A. R. Mandel.) *Amer. Jour. Physiol.*, 10, 45-56.

On the action of phlorhizin. (With P. G. Stiles.) *Amer. Jour. Physiol.*, 10, 67-79.

1904

Diabetes mellitus. Report of a case, including a new method of prognosis. (With A. R. Mandel.) *Jour. Amer. Med. Assn.*, 43, 241.

On the absence of a cane-sugar inverting enzyme in the gastric juice. *Proc. Amer. Physiol. Soc. Amer. Jour. Physiol.*, 10, 20.

Stoffwechselbeobachtungen an einem Falle von Diabetes mellitus, mit besonderer Berücksichtigung der Prognose. (With A. R. Mandel.) *Deutsches Archiv. klin. Med.*, 81, 472-492.

Report on the metabolism of a case of diabetes mellitus. (With A. R. Mandel.) *Amer. Med., Phila.*, 6, 790.

1905

Theories of metabolism. *Science*, n. s., 22, 6-12.

1905-06

A law of growth reiterated. *Proc. Amer. Physiol. Soc.*, p. xvii.

On the intermediary metabolism of lactic acid. (With A. R. Mandel.) *Yale Med. Jour.*, 12, 830-832.

1906

Lactic acid in metabolism with a note on the cause of fatty changes in the organism. (With A. R. Mandel.) *Jour. Amer. Med. Assn.*, 47, 1804-06.

Lactic acid in intermediary metabolism. (With A. R. Mandel.) *Amer. Jour. Physiol.*, 16, 129-146.

1907

Metabolism in phosphorus poisoning. *Amer. Jour. Physiol.*, 19, 461-467.  
Prepared and predigested foods. *Jour. Amer. Med. Assn.*, 49, 201.

1908

Modern conceptions regarding chemical regulations of function. *N. Y. State Jour. Med.*, 8, 302-4.

The production of sugar from glutamic acid ingested in phlorhizin glycosuria. *Amer. Jour. Physiol.*, 22, 174-178.

The influence of cold and mechanical exercise on the sugar excretion in phlorhizin glycosuria. *Amer. Jour. Physiol.*, 22, 163-173.

Carl von Voit. *Science*, n. s., 27, 315-16.

Eine Erklärung der spezifisch-dynamischen Wirkung der Eiweisstoffe. *Zentralb. Physiol.*, 21, 861.

The behavior of alanin in metabolism. (With A. I. Ringer.) *Proc. Soc. Exp. Biol. Med.*, 6, 4.

1909

Animal experimentation and diabetes. One of a series of articles issued under the auspices of the Committee on Experimental Medicine of the Medical Society of the State of New York.

Metabolism in diabetes. *Harvey Lectures. Arch. Int. Med.*, 3, 1-22.

Medical Education—A plea for the development of leaders. *Jour. Amer. Med. Assn.*, 52, 1229-30.

1910

The fate of the amino acids in the organism. *Jour. Amer. Chem. Soc.*, 32, 671.

Ueber die Entstehung von Dextrose aus Aminosäuren bei Phlorhizinglykosurie. (With A. I. Ringer.) *Zeitschr. physiol. chem.*, 66, 106-119.

A plea for hospital reorganization. *Jour. Amer. Med. Assn.*, 54, 1421-22.

Metabolism in diabetes. *Jour. Amer. Med. Assn.*, 55, 2105-7.

An attempt to discover the cause of the specific dynamic action of protein. *Proc. Soc. Exp. Biol. Med.*, 7, 136-37.

1910-11

The influence of cold baths on the glycogen content of man. *Amer. Jour. Physiol.*, 27, 427-37.

On the question whether dextrose arises from cellulose in digestion. *Amer. Jour. Physiol.*, 27, 467-68.

1911

- The chemical and energy transformations in the dog after the ingestion of different quantities of meat. (With H. B. Williams and J. A. Riche.) *Proc. Soc. Exp. Biol. Med.*, 8, 61-62.
- Christian Archibald Herter. *Science*, n. s. 33, 846-47.
- Metabolism in diabetes mellitus. (F. G. Benedict and E. P. Joslin, reviewed by G. Lusk.) *Science*, n. s. 33, 433-4.
- Alcohol—its use and abuse. *Pop. Sci. Mo.*, April.
- The use of sodium benzoate as a preservative of food. *Science*, n. s. 34, 759-60.
- The Remsen Board and Dr. Wiley. *Med. Rec.*, Dec. 9.
- A method of removing glycogen from the human subject. *Proc. Amer. Physiol. Soc.*, 26, 22.

1912

- Phlorhizinglykosurie. *Die Ergebnisse der Physiologie*, 12, 315-92.
- Calorimetric observations. *Med. Rec.*, 82, 925-28.
- Animal Calorimetry. II Paper. Metabolism of the dog following the ingestion of meat in large quantities. (With H. B. Williams and J. A. Riche.) *Jour. Biol. Chem.*, 12, 349-76.
- Note on "A case of pancreatic diabetes mellitus" by H. O. Mosenthal. *Arch. Int. Med.*, 10, 122.

1912-13

- Animal Calorimetry. III Paper. Metabolism after the ingestion of dextrose and fat, including the behavior of water, urea, and sodium chloride solutions. *Jour. Biol. Chem.*, 13, 27-47.
- Animal Calorimetry. V Paper. The influence of the ingestion of amino-acids upon metabolism. *Jour. Biol. Chem.*, 13, 155-83.
- Animal Calorimetry. VI Paper. The influence of the mixtures of food-stuffs upon metabolism. (With J. A. Riche.) *Jour. Biol. Chem.*, 13, 185-207.
- Animal Calorimetry. VII Paper. The metabolism of a dwarf. (With F. H. McCrudden.) *Jour. Biol. Chem.*, 13, 447-54.

1913

- The influence of foodstuffs and their cleavage products upon heat production. *Trans. XV Intern. Cong. Hyg. and Demog.*, II, 400.
- The cause of the specific dynamic action of protein. *Arch. Int. Med.*, 12, 485-7.
- The alleged influence of the adrenals and the thyroids upon diabetic metabolism. *Proc. XVII Intern. Cong. Med.*, II, p. 13.
- Food at fifty cents a day. *N. Y. Evening Post*, Feb. 8.
- Medical education in the United States. *Science*, n. s. 38, 491-499.

1913-14

- The specific dynamic action of levulose, glycolol and alanin in phlorhizin glycosuria. *Proc. Soc. Exp. Biol. Med.*, 11, 168.

The influence of epinephrin on carbohydrate metabolism. (Abstr.) *Proc. Soc. Exp. Biol. Med.*, 11, 49.

1914

The specific dynamic action of the foodstuffs. *Jour. Amer. Med. Assn.*, 63, 824-27.

Food from the standpoint of energy. *Med. Rec.*, 85, 740-1.

Animal Calorimetry. VIII Paper. The alleged influence of the adrenals on diabetic metabolism. (With J. A. Riche.) *Arch. Int. Med.*, 13, 673-81.

1915

Animal Calorimetry. *Johns Hopkins Hosp. Bull.*, 26, 10-12.

The respiratory quotient in diabetes. *Proc. Soc. Exp. Biol. Med.*, 12, 125-6.

On the proposed reorganization of departments of clinical medicine in the United States. *Science*, n. s. 41, 531-4.

The influence of food on metabolism. Presidential Address, Proceedings of the American Society of Biological Chemists. *Jour. Biol. Chem.*, 20, vii-xvii.

Animal Calorimetry. XI Paper. An investigation into the causes of the specific dynamic action of the foodstuffs. *Jour. Biol. Chem.*, 20, 555-617.

Animal Calorimetry. XII Paper. The influence of the ingestion of fat. (With J. R. Murlin.) *Jour. Biol. Chem.*, 22, 15-41.

The calorimeter as the interpreter of the life processes. *Science*, n. s. 42, 816-9.

Clinical Calorimetry. I Paper. A respiration calorimeter for the study of disease. *Arch. Int. Med.*, 15, 793-804.

Clinical Calorimetry. VIII Paper. On the diabetic respiratory quotient. *Arch. Int. Med.*, 15, 939-44.

1916

Clinical Calorimetry. XVIII Paper. The number of places of significant figures in the data of metabolism experiments. (With F. C. Gephart and E. F. Du Bois.) *Jour. Biol. Chem.*, 27, 217-23.

Food economies. *Jour. Wash. Acad. Sci.*, 6, 387-96.

Intermediary metabolism in diabetes. *Trans. Coll. Phys. Phila.*, 38, 244-48.

Acidosis. *Jour. Amer. Med. Assn.*, 66, 144-45.

1916-17

Influence of diet on the heat production during mechanical work in the dog. *Proc. Soc. Exper. Biol. Med.*, 14, 92.

1917

Intermediary metabolism in diabetes. *Amer. Jour. Med. Sci.*, 153, 40-43.

Animal Calorimetry. XIII Paper. The interrelation between diet and body condition and the energy production during mechanical work. (With R. J. Anderson.) *Jour. Biol. Chem.*, 32, 421-45.

- Clinical Calorimetry. XXIV Paper. Metabolism in three unusual cases of diabetes. (With F. C. Gephart, J. C. Aub and E. F. Du Bois.) *Arch. Int. Med.*, 19, 908-30.
- Food for a day. The Day's Food in War and Peace, a symposium published by the U. S. Department of Agriculture, Women's Committee, Council of National Defense, pp. 20-24.
- Food values. *Science*, n. s. 45, 345.
- Economy in diet. Reports and Address of the Proceedings of the National Conference of Social Work, Chicago, No. 94, p. 231.
- Food in war time. *Sci. Mo.*, 20, 298.
- What to eat in war time. *World's Work*, 34, 446.
- Calories in common life. *Med. Clin. N. Amer.*, N. Y. Number, i, No. 3, 481.

1918

- The fundamental requirement of energy for proper nutrition. *Jour. Amer. Med. Assn.*, 70, 821-24.
- Obituary of Theodore Caldwell Janeway. *Amer. Jour. Med. Sci.*, 155, 4.
- The Scientific Aspect of the Inter-Allied Food Situation. Wesley M. Carpenter Lecture delivered before the N. Y. Academy of Medicine, October 3, 1918. Published under title "Science and the World's Food," *Scribner's Magazine*, 64, 710.
- Food for our allies. *Amer. Mus. Jour.*, 18, 629.
- Science and medical teaching. *Science*, n. s., 48, 629.
- Animal Calorimetry. XV Paper. Further experiments relative to the cause of the specific dynamic action of protein. (With H. V. Atkinson.) *Jour. Biol. Chem.*, 36, 415-27.

1919

- Notions de physiologie de la nutrition. La ration d'entretien. Vie chère et la santé—Comment nous alimenter. Bernard Grasset, Paris, p. 25. (Avec collaboration de E. Janselme, G. Lusk, J. Renault, M. Labbe, M. Mignon, 104 pp.)
- The nutritional standards of adolescence. *Jour. Home Ec.*, 11, 281, 1919. Published as Nutrition of Adolescence. Report of Children's Bureau Conferences, Conference Series No. 1, Bureau Pub. No. 60, p. 256.
- Scientific Medicine—Yesterday and Tomorrow. *Jour. Amer. Med. Assn.*, 73, 181.
- A National Laboratory of Human Nutrition. *Science*, n. s., 50, 97.
- Calorimétrie comparée de l'ingestion de viande, d'acide lactique et d'alanine chez l'animal. Presented by Armand Gautier. *C. R. Acad. Sci.*, 168, 1012.
- Influence of lactic acid upon the metabolism of the dog. (With H. V. Atkinson.) *Proc. Soc. Exp. Biol. Med.*, 17, 14.
- On the problem of the production of fat from protein in the dog. (With H. V. Atkinson.) *Proc. Nat. Acad. Sci.*, 5, 246-8.

Animal Calorimetry. XVI Paper. The influence of lactic acid upon metabolism. (With H. V. Atkinson.) *Jour. Biol. Chem.*, 40, 79-89.  
A Medical School, in the War and After. *Science*, n. s., 50, 403-5. Editorial of this entitled "Activities of a Medical College." *Boston Med. Surg. Jour.*, 181, 22, 645.

The Eight Hour Day, a French Medical Interpretation. Translated from an article in *LA PRESSE MÉDICALE*. *Jour. Amer. Med. Assn.*, 73, 1717.

1920

The Manhattan Medical School. *Jour. Mus. Nat. Hist.*, 20, 142-3.  
Additional experiments showing the production of fat from protein. *Proc. Soc. Exper. Biol. Med.*, 17, 171-2.

Une édition américaine du traité élémentaire de chimie, de Lavoisier, publiée a Philadelphie en 1799. (With J. Le Goff.) *La Gazette des hôpitaux*, No. 41, p. 650, May.

1921

Some influences of French science on medicine. The Pasteur Lecture, Institute of Medicine of Chicago. *Proc. Inst. Med.*, Chicago, 3, 98-117.  
Fundamental ideas regarding the basal metabolism. *Jour. Amer. Med. Assn.*, 77, 250-2.

The physiological effect of undernutrition. *Physiol. Rev.*, 1, 523-52.

Animal Calorimetry. XVIII Paper. The behavior of various intermediary metabolites upon the heat production. *Jour. Biol. Chem.*, 49, 453-78.

Personal Reminiscences of Dr. Meltzer. In Memorial Number S. J. Meltzer, N. Y.

1922

De l'influence de la science française sur la médecine. (French translation of Pasteur Lecture.) Translated by P. Gerald. *Revue Scientifique*, No. 6, 177-88, March 25.

Animal Calorimetry. XXI Paper. The influence of morphine upon heat production in the dog. (With A. Chanutin.) *Jour. Pharmacol. Exp. Therap.*, 5, 359-73.

Animal Calorimetry. XXII Paper. The production of fat from protein. (With H. V. Atkinson and David Rapport.) *Jour. Biol. Chem.*, 53, 155-66.

The specific dynamic action of various food factors. *Medicine*, 1, 311-354.

Principes fondamentaux concernant le métabolisme basal. *Gaz. d. hôp.*, 95, 165-68.

On the mental life of a professor of medicine. (With F. Müller.) *Jour. Amer. Med. Assn.*, 79, 1870.

1923

Pasteur, the Man. *Science*, 57, 139.

Methods of the Carnegie Institution—A Note to the Editor of *Science*. *Science*, 57, 271-2.

Commercial cult of vitamins. *The Survey*, 49, 784-5.

1924

Animal Calorimetry. Paper XXIV. Analysis of the oxidation of mixtures of carbohydrate and fat. A correction. *Jour. Biol. Chem.*, 59, 41-2.

Diet and disease. *Amer. Jour. Pub. Health*, 14, 297-301.

On the constancy of the basal metabolism. (With E. F. Du Bois.) *Jour. Physiol.*, 59, 213-16.

1925

Die Energiequelle bei der Muskelarbeit. *Biochem. Zeitschr.*, 156, 334-42.  
Disappearance of Ketone Bodies in presence of unoxidized sugar in completely phlorhizinized dogs. (Introduced by G. Lusk.) M. Wierzuchowski. *Proc. Soc. Exp. Biol. Med.*, 22, 425-6.

Interpretation of disturbances in metabolism due to the glands of internal secretion. *Endocrinology*, 9, 213-28.

Review of Emile F. Terroine and Edgard Zunz's "La Metabolisme de Base." *Science*, 62, 1606, p. 331.

Mementoes of Lavoisier. Notes on a trip to Château de la Canière. *Jour. Amer. Med. Assn.*, 85, 16.

1926

Animal Calorimetry. XXXIV Paper. The influence of Glycyl-Glycine upon the respiratory metabolism of the dog. (With N. H. Plummer and H. J. Deuel.) *Jour. Biol. Chem.*, 60, 339-48.

The chemistry of physiology and nutrition. *Jour. Amer. Chem. Soc.*, Golden Jubilee Number, 48, 153-75.

The specific dynamic action of food. Pp. 36-40 of Jubilee volume in honor of Dr. Richet, entitled "A Charles Richet. Ses Amis, ses Collègues, ses Élèves," May 22, 1926, edited by Auguste Pettit.

Fifty years of progress in the chemistry of physiology and nutrition in the United States. *Jour. Amer. Chem. Soc.*, 48, 153.

The influence of Glycyl-Glycine on the respiratory metabolism of the dog. (With H. J. Deuel, Jr., and N. H. Plummer.) *Skand. Arch. Physiol. (Abstr.)*, 49, 173.

1927

An address on protein metabolism to the students of McGill University, Montreal, Canada. Edited by Sidney Bliss. *Canada Med. Assn. Jour.*, 17, 522-24.

Presentation of the Medal of the American Institute of Chemists. (With L. B. Mendel.) *Science*, 65, 555-8.

Memorial meeting for Dr. John Howland. *Johns Hopkins Hosp. Bull.*, 41, 311-321.

1928

On diabetes. *New England Jour. Med.*, 199, 826-9.

1929

- Russell Henry Chittenden. *Ind. Eng. Chem.*, 21, 91-94.  
 Metabolism background. *Leopoldina (Amerika-Band)*. No. 4, 191-200.  
 The Thirteenth International Physiological Congress, Boston, August 19-23, 1929. *Sci. Mo.*, 28, 565-69.

1930

- Animal Calorimetry, XXXIX Paper. Specific dynamic action in the normal and phlorhizinized dog. (With W. H. Chambers.) *Jour. Biol. Chem.*, 85, 611-26.  
 Science and life. *Science*, 71, 271-75.  
 The Harvey Society. *Science*, 71, 627-9. Also, Harvey Lectures, 25, 205, 1929-30.  
 Science, work and criticism. *New England Jour. Med.*, 202, 1035-38.

1931

- Editorial review. The specific dynamic action. *Jour. Nutr.*, 3, 519-30.  
 Die spezifisch-dynamische Wirkung der Nahrungsstoffe. *Ergeb. Physiol.*, 33, 103-136.  
 Carl von Voit, master and friend. *Ann. Med. History*, n. s., 3, 582-94.

1931-32

- Animal Calorimetry. XLI Paper. The influence of phlorhizin glycosuria on the metabolism of dogs after thyroidectomy. (With Margaret Dann and W. H. Chambers.) *Jour. Biol. Chem.*, 94, 511-27.

1932

- To Lafayette B. Mendel. *Yale Jour. Biol. Med.*, 4, 369.  
 Contributions to the science of nutrition. A tribute to the Life and Work of Max Rubner. *Science*, 76, 129-135.

#### BOOKS—GRAHAM LUSK

- The Chemistry of the Animal Body. Edited by W. H. Howell. American Textbook of Physiology, Vol. I, 2nd edition, Philadelphia, W. B. Saunders, 1900.  
 The Elements of the Science of Nutrition. Philadelphia, W. B. Saunders Co., 1906. 1st edition, 326 pp.  
 The Elements of the Science of Nutrition. Philadelphia, W. B. Saunders Co., 1909. 2nd edition, 402 pp.  
 Ernährung und Stoffwechsel. (Translation by Dr. Hess.) Wiesbaden, J. F. Bergmann, 1910, 368 pp.  
 The Fundamental Basis of Nutrition. New Haven, Yale University Press, 1914, 62 pp.  
 Introduction to "Analysis and Cost of Ready-to-Serve Foods" by F. C. Gephart. Chicago, Press of the American Medical Association, 1915. 81 pp.

- The Elements of the Science of Nutrition. Philadelphia, W. B. Saunders Co., 1917, 3rd edition, 641 pp.
- Food in War Time (Calories in Common Life and Food in War Time, published separately in 1917). Philadelphia, W. B. Saunders Co., 1918, 46 pp.
- Food and Reconstruction. Chapter 23, Part V of America and the New Era, a Symposium on Social Reconstruction, edited by Elisha Friedman. New York, E. P. Dutton, 1920.
- A History of Metabolism. Introductory chapter to Vol. III Endocrinology and Metabolism. Edited by Lewellys F. Barker, R. G. Hoskins and H. O. Mosenthal. New York and London, D. Appleton & Co., 1922.
- Memories of the Food Situation in 1918. Published in Essays and Studies in Honor of Margaret Barclay Wilson. Columbia University Press, 1922, pp. 64-74.
- Fundamental Basis of Nutrition. New Haven, Yale University Press, 1924. 2nd edition. London: Humphrey Milford, Oxford University Press.
- The Physiology of the Thyroid Gland. pp. 13-16 in chapter on "The Thyroid Gland" in Glandular Therapy. Series of articles prepared under auspices of the Council on Pharmacy and Chemistry, American Medical Association, Chicago, 1925, 89 pp.
- Problems of Metabolism. Chapter of Lectures on Nutrition pub. by Mayo Foundation, embracing a series of lectures given at the Mayo Foundation and the Universities of Wisconsin, Minnesota, Nebraska, Iowa and Washington (St. Louis) during 1924-1925 by F. G. Benedict, Graham Lusk, E. F. Du Bois, A. V. Hill, E. V. McCollum and H. M. Evans. Philadelphia, W. B. Saunders Co., 1925, 243 pp.
- Elements of the Science of Nutrition. Philadelphia and London, W. B. Saunders Co., 1928. 4th Edition, 844 pp.
- Nutrition. Clio Medica Series, X, New York, Hoeber, 1933, 129 pp.

#### OTHER REFERENCES

1. Lusk, William Thompson: War letters of. Privately printed, New York, 1911.
2. ———: (Lusk, G.): Physiology at the Harvard Medical School, 1870-1871. Boston Med. and Surg. Jour., 167, 921-22, 1912.
3. Coleman, W. and Wright, A. M.: Obituary of William Chittenden Lusk, 1868-1934. Bull. N. Y. Acad. Med., 11, 160-63, 1935.
4. Du Bois, E. F.: Basal Metabolism in Health and Disease. Lea and Febiger, Philadelphia, First Edition, 1924; Second Edition, 1927; Third Edition, 1936.
5. Gephart, F. C. and Du Bois, E. F.: Clinical Calorimetry, IV. Determination of basal metabolism of normal men and the effect of food. Arch. Int. Med., 15, Part II, 835-67, 1915.

6. Gies, W. G.: The Society for Experimental Biology and Medicine. Amer. Med., 5, 797-99, 1903.
7. Anon.: Proc. Soc. Exper. Biol. Med., 1, 53, 1903-04.

# TRIBUTES

8. Anon.: Graham Lusk: Jour. Biol. Chem., 73, preceding p. 605, 1932.
9. Wallace, G. B.: Obituary of Graham Lusk. Bull. N. Y. Acad. Med., 8, 631-34, 1932.
10. Anon.: Obituary of Professor Lusk. London Times, July 21, 1932.
11. LeGoff, J. M.: Graham Lusk. Gaz. d. hop. 105, 1486, 1932.
12. Murlin, J. R.: Graham Lusk, A brief review of his work. Jour. Nutr., 5, 526-538, Sept. 1932.
13. Müller, F.: Graham Lusk. München. med. Wchnschr., 1932, 79, 1572-73.
14. Mendel, L. B.: Professor Graham Lusk. Sci. Mo., 35, 280-83, 1932.
15. ———: Graham Lusk. Jour. Biol. Chem., 98, 1, 1932.
16. Du Bois, E. F.: Graham Lusk. Science, 76, 113-115, 1932.
17. ———: Obituary of Graham Lusk, 1866-1932. Ergeb. Physiol., 35, 9-12, 1933.
18. LeGoff, J. M.: Graham Lusk. Bull. Soc. chim. biol., 15, 147, 1933.
19. Du Bois, E. F.: A tribute to Graham Lusk. Jour. Amer. Dietet. Assn., 8, 396-402, 1933.
20. Cathcart, E. P.: Graham Lusk—1866-1932. Obituary Notices of Fellows of the Royal Society, No. 2, 143-46, 1933.
21. Hartwell, J. A.: Graham Lusk as a Friend. Address given at a memorial meeting for Graham Lusk, New York Academy of Medicine, December 10, 1932. Privately printed, Baltimore, 33 pp., 1933.
22. McCann, W. S.: The influence of Graham Lusk upon his students. *Ibid.*, p. 10.
23. Joslin, E. P.: The influence of Graham Lusk in advancing knowledge in the field of diabetes. *Ibid.*, p. 16.
24. Chittenden, R. H.: The influence of Graham Lusk on the Science of Nutrition. *Ibid.*, p. 22.
25. Swift, H. F.: The influence of Graham Lusk on medical education. *Ibid.*, p. 29.
26. Carlson, A. J.: Graham Lusk. *Ibid.*, p. 38.





*Elihu Thomson*

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OF

ELIHU THOMSON

1853–1937

BY

KARL T. COMPTON

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## ELIHU THOMSON

1853-1937

BY KARL T. COMPTON

For one destined to apply his genius largely toward harnessing electricity for the work and comfort of man, the decade beginning with 1850 was a timely period in which to be born. The preceding half century had witnessed the fundamental discoveries which underlie the utilization of electricity, and imaginative minds had begun to direct these discoveries into the broad channels of practical and commercial employment.

In the development of the electrical art this first half of the Nineteenth Century was a remarkable fifty years, and because it provided the foundation for the practical achievements which came in the second half, a review of it helps to give perspective to this memoir on Elihu Thomson.

The century opened auspiciously with Volta's discovery of the voltaic cell, and with the demonstration by Nicholson and Carlisle of electrolysis. In 1820 Oersted announced his discovery that an electric current has the power to deflect a magnetic needle. In this same year Ampere brilliantly elucidated Oersted's discovery by giving mathematical expression to the forces produced by electric currents. Six years later Ohm announced the formulation of his law that current is proportional to the electromotive force, and twenty years later Gauss and Weber invented an acceptable system of electrical and magnetic units.

Meanwhile, Faraday had begun the epocal researches which were to lay the foundations of electrical engineering. In 1821 he had succeeded in making a wire revolve about a magnet and a magnet about a wire, and ten years later, almost simultaneously with Henry in America, he made the great discovery underlying almost all electrical machinery—electromagnetic induction. This led him to the mechanical production of a steady electric current by revolving a copper disc between the poles of a magnet. Here, at last, in embryo, was the machine which ultimately would generate in one year in the United States alone 120 billion kilowatt hours of electric power.

Minds with a practical bent were quick to follow the road which Faraday and Henry had pointed out, but they found the going slow. By 1850, the electric motor had been demonstrated, the commutator had been devised, the electric arc had been experimentally used for lighting, and efforts had been made to drive boats, buggies, and locomotives by electricity. But the conquest of electric power was still thwarted by practical difficulties; only in the form of the telegraph and a few other devices had electricity been put to work effectively. Efforts to obtain a reliable mechanical source of electric power languished.

It was during this stage in the development of the electrical art that Elihu Thomson was born in 1853, and it was not until he had embarked upon his professional career at the tender age of 17 and was ready to join the creative thrust that the drive toward economic utilization of electric power had really begun to gain ground rapidly. In 1875, five years after Gramme had built his ring-wound armature, and along with Siemens had made the dynamo a practical machine, Thomson had built a dynamo and by 1879 he had invented and patented a three-coil arc dynamo—the first three phase generator. He thus early took prominent place in the brilliant group, including Brush, Edison, Siemens, Stanley, Tesla, Van Depoele, Weston, and others, which was to solve the problem of generating adequate current. The electrical tide was approaching its flood and Thomson was ready—with consequences enormously important to the development of the electrical industry.

The young man who thus auspiciously began his career in Philadelphia was born in Manchester, England, on March 29, 1853, of a Scotch father, Daniel, and an English mother, Mary Rhodes. Elihu was the second son of the family which ultimately was to total eleven children, six boys and five girls. Four years after Elihu's birth, the panic of 1857 struck England and his parents, moved by the resulting scarcity of work, decided to emigrate to America, which they did in 1858, settling in Philadelphia. Elihu early showed signs of exceptional ability. When his parents felt the appropriate time had arrived for them to teach him his alphabet, they were astonished to discover that the

youngster, now five years old, not only knew the letters but could recite the alphabet both forwards and backwards.

Elihu's father was a gifted mechanic and his work led naturally to Elihu's interest in technical and industrial arts. As he himself has recalled,

"A great many of the industrial establishments, on account of my father's work as engineer and machinist, were open to me. I was thus able to witness as a boy many of the industrial processes going on, both in chemical work and also in mechanical constructions, in which I was always interested even from the start. The literature which was available to me at home was chiefly the 'Imperial Journal of Arts, Sciences and Engineering', of which there were two volumes, which I studied actively. Evidently my tastes had already been formed and were, perhaps, to a certain degree, hereditary, intensified by my father's occupation and that of several of my uncles, who followed mechanical pursuits. I was constantly endeavoring to imitate, in a small way, the processes and operations which I saw going on around me. Thus, at about the age of ten or eleven, I constructed small models of cupola furnaces with fan blowers for furnishing the blast and actually succeeded in melting cast iron, hoping to be able to get enough iron to make castings. In this, I was not successful, as the iron melted was not in sufficient quantity to run into a mold. I was, however, always interested in what was going on around me, such as the laying of water pipes and gas pipes in the streets, the building of sewers, etc., spending hours in watching the operations. I remember that I was constantly imitating on a small scale, or by drawings, operations mostly of an engineering nature which I saw going on about me. What I couldn't actually make, I contented myself by drawing. During the latter part of the period of the Civil War, I often visited the Philadelphia Navy Yard and operated a donkeyengine during the noon hour, so that the men need not stop work. This engine was used for the boring out of the propeller holes of two ships then under construction in the yard. One was an iron-clad cruiser called 'The Tonawanda' belted with four inches of iron on a wooden hull, and the other was a high powered ship intended for chasing blockade runners and named 'The Chattanooga'. As a boy of about fourteen years of age, I had access to a large chemical works, where sulphuric, nitric and hydrochloric acid were made, and where paints and pigments were a

large portion of the production. Needless to say, I understood the processes from my own chemical reading.”<sup>1</sup>

Elihu entered the public schools of Philadelphia at the age of six and by the time he was eleven years of age, he was ready to enter the Boys' Central High School. Under existing regulations, he could not be accepted until he was thirteen, and because Elihu was not particularly strong, his parents seriously considered the recommendation of the grammar school principal that he give up studying entirely for two years and attempt to build up his physique. To this suggestion Elihu reacted promptly and violently, telling his parents that he would as soon die as to give up his books. The parents capitulated, and young Thomson embarked on a period of reading and a program of gadget making and youthful experimentation. He built a static machine from a wine bottle, small condensers, Leyden jars, a pair of telegraph instruments, and voltaic cells, and he assembled a collection of chemicals adequate to carry out many processes and reactions.

In February, 1866, he was finally admitted to the Central High School, even though he lacked several weeks of having attained the required age. Four years later he was graduated as fourth honor man and accepted employment in a commercial laboratory where analyses were made of iron ore and other minerals. He remained in this post for about six months and then returned to Central High School in the fall as “Adjunct to the Department of Chemistry” at a salary of \$500 per year.<sup>2</sup>

One of the senior professors whom he assisted in this post was Edwin J. Houston, who held the chair of Physical Geography and Natural Philosophy, and the two were soon engaged in collaborative investigations which led to a long partnership. The first publication growing out of their research was a paper “On a New Connection for the Induction Coil,” contributed by Professor Houston to the June, 1871, issue of the *Journal of the*

<sup>1</sup> From an unpublished letter, dated January 26, 1933, in the files of the National Academy of Sciences.

<sup>2</sup> The Philadelphia Period in the Life of Professor Elihu Thomson by John Louis Harney. *The Barnwell Bulletin* of Central High School, February, 1939.

*Franklin Institute.* The paper contained an account of Thomson's observations of sparks drawn from grounded waterpipes during the operation of a nearby induction coil. Although he did not recognize the significance of the evidence at the time, he had clearly observed the propagation of electrical waves through space. When, in 1875, Edison announced a new "etheric" force which he described as non-electrical, Professor Thomson was primed to dispute his conclusions, for he wrote later

"I had proposed to Houston that we carry on these experiments and show definitely that the so-called 'etheric' force that Edison had announced in the papers was merely an electrical phenomenon. At this time I took upon myself the enlargement of the scale of the experiments, so as actually to obtain a very definite result. This was carried out, as follows, in 1875. A 6-inch spark Ruhmkorff coil was set up with one terminal connected by a wire about 5 feet long to a large tin vessel mounted on a glass jar on the lecture table. When the coil was in operation, sparks were allowed to jump across the terminals of the coil itself, these sparks being about  $1\frac{1}{2}$  inches to 2 inches long and having the character of condenser sparks. When the coil was in action, I explored the whole building throughout the several floors and then went up to the top of the building to the observatory, where Professor Snyder had charge of the astronomical instruments. It was found that tiny sparks could be obtained from metal objects wherever they were, in the cases or outside, from the door-knobs or from apparatus, by the simple expedient of shading from the light and detecting the tiny sparks with a pointed pencil by applying it, say, to the door-knob. I recognized clearly that this was a manifestation of electric waves passed through space, and I also understood that a system of communication might readily be based thereon."<sup>3</sup>

A description of this experiment was communicated to the Franklin Institute by Professor Houston and printed in its *Journal* for January, 1876. With the exception of Joseph Henry's experiments, which were unpublished, here was the first experimental demonstration of the validity of Maxwell's theory, and here, too, was an example of Professor Thomson's extraordinary intuition anticipating the wireless transmission of signals over a decade before Hertz demonstrated electro-

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<sup>3</sup> Unpublished notes of Professor Thomson in the files of J. A. McManus, General Electric Company, Lynn, Mass.

magnetic waves and twenty odd years before Marconi received his patent on "telegraphy without wires".

Again in Thomson's nineteenth year, the *Journal of the Franklin Institute*, August, 1871, carried an account, written jointly by Thomson and Houston of further original work by Thomson. This paper, "On the Change of Color Produced in Certain Chemical Compounds by Heat," was a pioneer discussion of this phenomenon. His next important paper, "On the Inhalation of Nitrous Oxide, Nitrogen, Hydrogen, and other Gases and Gaseous Mixtures" appeared in the *Philadelphia Medical Times*, November 15, 1873, and foreshadowed his later work on the use of helium in diving and caisson work.

By 1877 Thomson was swinging into his full stride. He had received the Master of Arts degree from his institution and been appointed Professor of Chemistry and Mechanics. His capacity to work productively in a variety of fields had been amply demonstrated by creative work in both chemistry and physics, and by such avocational activities as lens grinding and the construction of a pipe organ with electropneumatic key action. He had, during a series of successful lectures at the Franklin Institute, anticipated the system of electric-welding he was later to patent, he had conceived the idea of a cream separator, and he had described the operation of tuning one electrical circuit to another.

Thomson regarded his "more serious interest in electrical applications" <sup>4</sup> as beginning in 1878 with a series of tests on dynamos then in commercial use. This report had been preceded in the *Journal of the Franklin Institute* by papers on the relaying of the telephone and on "A New System of Electric Lighting and a New Form of Electric Lamp," and it was followed in 1879 by "Circumstances Influencing the Efficiency of Dynamo Electric Machines" published jointly with Professor Houston in the *Proceedings* of the American Philosophical Society. This paper, as did the report to the Franklin Institute, emphasized the advantage of low internal resistance in a dynamo as compared to the resistance of the external circuit.

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<sup>4</sup> "Pioneer Investigations on Dynamo Machines Fifty Years Ago," by Elihu Thomson. The *Journal of the Franklin Institute*, July, 1928.

It was in 1879 that he and Houston built a dynamo with three-phase winding. This machine, patented in 1880 and now at the Smithsonian Institution, was known as the "bakery machine" because of its use for lighting a large bakery in Philadelphia. "This is the machine," Thomson once noted, "upon which the Thomson-Houston Electric Company was based. . . . I think this is a very important invention, inasmuch as the great power generators of today are three-phase dynamo machines with three-phase armature winding. . . ." <sup>5</sup>

Having made fundamental improvements in the dynamo, Thomson and Houston, prompted by the commercial application of arc lighting by Brush, rapidly rounded out a complete and reliable arc-lighting system. They devised a constant current regulator (1881), an air blast method to extinguish or prevent the arc tending to occur when an electric circuit is opened (1882), and the magnetic blow-out (1883) which employs a magnetic field to extinguish an arc.

Of this arc-lighting development Dr. Dugald C. Jackson, the well-known electrical engineer, has said:

"Arc lighting has largely been superseded by later forms of electrical illumination, but I am personally inclined to put forward this invention of the automatically regulated dynamo for arc-lighting service as one of Thomson's most important, on account of its influence on his own work and the development of his opportunities. The invention was made when he was still in his twenties. It was carried through substantially on his own responsibility except for meager financial aid, and drew out at this early age, at least in some degree, those qualities of originality, courage, resourcefulness, far-sighted thinking and powers of experiment which were so notably the foundation for his distinguished and productive career." <sup>6</sup>

For similar reasons I have dwelt in detail on Professor Thomson's Philadelphia days, particularly on his work at Central High School. By the time he resigned from the school in 1880, he had unmistakably demonstrated his wide-ranging genius, and in his

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<sup>5</sup> Unpublished notes of Professor Thomson in the files of J. A. McManus, General Electric Company, Lynn, Mass.

<sup>6</sup> Address of Dugald C. Jackson at the meeting in commemoration of the life and work of Elihu Thomson, February 16, 1939. In the files of the American Philosophical Society, Philadelphia.

work there are to be found the seeds of his later achievements. Here it was, too, that he developed his life-long interest in education and that fondness for teaching which led him throughout his life to cherish the title "Professor" above all others. Of his early developed gifts as a teacher there is direct testimony from Dr. Edwin W. Rice, Jr., a student of Professor Thomson's during the Central High days, later his assistant, and ultimately the President of the General Electric Company.

"To me he has been 'My Professor' ever since I first met him away back in the year 1876 in the Central High School of Philadelphia. He was a youthful professor of chemistry in his twenty-third year and I was a young student of fourteen. I was full of eagerness to learn; he was equally keen to teach. My discovery of Professor's genius occurred years before he had become famous; before he had started on his career of invention which was to astonish the world. He was at that time an obscure young teacher unknown to the world, but to me he was as wonderful then as he is today. I therefore feel that I may have a good claim to call him 'My Professor'. The High School was to me a wonderful new world; full of books and bottles; of magnets and batteries, and topped by a great dome containing a marvelous telescope. It was there, as I have said, that I first met Professor Thomson. On my side it was a case of love at first sight, and what a discovery; what a mine of knowledge, ready to be explored, as willing to give as I was to receive its richness. It is my recollection, that there was no question that I asked to which I failed to obtain a satisfactory reply, expressed in language that I could understand. It was to me a new and glorious experience! Encouraged by his friendly attitude I summoned up courage to waylay him at recess, and my joy knew no bounds on the occasion when he invited me to remain after school and continue our talk and to be shown some new scientific discovery. . . ."

Professor Thomson resigned from Central High School to become "electrician" for the American Electric Company, a firm organized early in 1880 at New Britain, Conn., to control the Thomson-Houston patents. Two years later Thomson, at the suggestion of Charles A. Coffin of Lynn, Mass., formed the Thomson-Houston Company to take over the assets of the New

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<sup>7</sup> "My Professor," by Edwin W. Rice, Jr. Elihu Thomson, Eightieth Birthday Celebration at the Massachusetts Institute of Technology, March 29, 1933. The Technology Press, Cambridge, Mass.

Britain Company, and in 1883 the business was moved to Lynn. With Coffin assuming the burden of finance and management, Thomson was free to give undivided attention to research and technical development, and for the first time he was able to surround himself with competent assistants. The result of this happy arrangement was one of the most extraordinary records of technical achievement in the history of the electrical industry.

Founded in the period when Edison was demonstrating the commercial possibilities of electricity with his "Jumbo" dynamos, the company grew rapidly. In 1884 it employed 184 workers, but by 1892, when it was merged with its competitor, the Edison General Electric Company of Schenectady, the number had grown to 4000.<sup>8</sup> The result of the merger was the General Electric Company, with Coffin as President and Rice, who had been manager of the Lynn plant, as Vice-President and Technical Director. Not the least of Professor Thomson's contributions to the success of this great industrial organization was his demonstration of the value of industrial research.

Returning to the record of Professor Thomson's inventions, we find him in 1885 applying his magnetic blowout to lightning arresters. This fundamental method of breaking electric currents became the foundation for automatic circuit breakers and for controllers of electric cars and trains.

The basic idea of his lightning arrester derived from an accurate knowledge and study of scientific phenomena involved in the discharge of electricity through gases. A transmission line, of course, has to be insulated from the earth by insulators adequate to prevent spark-over at the voltages used. If, however, the line is struck by lightning or an abnormally large electric surge passes through it, a spark may pass around the insulation, and it is a peculiarity of sparks through air that when once the insulation of the air is broken down by a spark there is literally no limit to the amount of current which can flow. Thus these sparks frequently cause serious short circuits.

Professor Thomson's discovery consisted in placing the in-

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<sup>8</sup> "Professor Thomson and the Development of the Lynn Electrical Industry" by J. A. McManus, Tercentenary edition "Greater Lynn," June 1929, Lynn Chamber of Commerce.

sulator between the poles of a magnet, with the result that the spark or arc which might be produced was acted on by electrical forces in such a way as to elongate it in the form of a bow which became more and more extended until it finally became so long that it went out. This principle is of just as great importance today as ever and is the foundation of many recently improved schemes for the switching of very large currents.

Again in these early days and long before the importance of it was understood, Thomson had outlined the now universally used method of transmitting alternating current by transformers. He had written out a description of the system in 1878 and set up a working model at the Franklin Institute in 1879, but his patent application was not filed until 1885. After an unusually strenuous history in the Patent Office because of interferences with the work of Gaulard, Gibbs, Brush and others, the patent did not issue until 1902. When it did issue it covered every alternating current distribution system in the country, and it is not surprising, therefore, that the courts subsequently held the patent invalid.

One of the reasons why Thomson delayed his application for this celebrated patent on alternating current distribution was his fear that the system would be dangerous when reduced to practice; the insulation of the transformer might break down and the high voltage of the primary would appear in the secondary circuit. It was not until he discovered in 1885 a way to avoid the danger, chiefly by grounding the secondary in the transformer, that he was willing to see the distribution system put into use.

In the further development of alternating current machinery he devised constant current transformers embodying the magnetic leakage shunt (1889), and a movable secondary (1894), which could be adjusted, in relation to a fixed primary, to give constant current output. Again, in the direction of increasing the power capacity of transformers, he obtained patents in 1890 covering the cooling of transformers by oil immersion and by air. He further called attention to the deleterious effect of moisture in the oil, an effect the full significance of which experts of insulation are only now beginning adequately to realize.

I pass now to two of the most important and characteristic of Professor Thomson's discoveries. The first of these is that process of electrical welding (1886), whereby the welded surfaces were fused and united by the heat developed on account of the resistance in the contact between them. This method of welding has come into enormous use in industry and the indications are that it will be even more used in the near future. As examples in widely different fields may be mentioned the welding of seamless metal tubing, the attachment of filaments and other electrodes in incandescent lamps and vacuum tubes, and the fastening together of many of the parts of automobiles. In the former of these applications it may be interesting to know that a single manufacturer had manufactured, a few years ago, about 24,000 miles of bedstead tubing by this process in a single year.

Professor Thomson described the genesis of this invention as follows:

"While preparing a lecture on Electricity (one of a course of five) at the Franklin Institute at Philadelphia, early in 1877, I had the temerity to pass the discharge of a Leyden battery through the fine wire secondary of a Ruhmkorff induction coil, while the primary coil of quite coarse wire had its terminals resting together in contact. As the Ruhmkorff was my own, one I had made, I could take the risk of breaking down the insulation. On the passage of the condenser spark of about 35 mm. length, a bright flash appeared at the ends of the heavy primary in contact, and I afterward found them firmly welded together.

"This suggested to me the possibility of electric welding, and later, about 1885, as soon as opportunity afforded, I built the first electric welder,<sup>9</sup> using a transformer to step down to a very short and heavy secondary between the terminals of which, by suitable clamps, the pieces to be welded were held in juxtaposition or contact. The first trials of this apparatus were highly successful, and welds were made not only between pieces (bars) of the same metal, but many different metals were so united."

Professor Thomson was not the first to utilize an arc in welding. There was some previous art, such as that of Slavianoff

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<sup>9</sup> "Electric Welding," by Elihu Thomson, the *Electrical World*, December 25, 1886.

and DeMeritens, but the DeMeritens patent, which was fundamental, was bought on advice of Professor Thomson by the Thomson Electric Welding Company in the early days, and had arc welding developed within the life of the patent, that company would have controlled the arc as well as the electric resistance welding art.

Again, one of Professor Thomson's most fundamental discoveries was the principle of dynamical repulsion between a primary and secondary coil. This can be demonstrated by a variety of interesting lecture experiments, most of which were suggested and shown first by Professor Thomson himself. One of these experiments still serves as a spectacular demonstration for popular science lectures and for elementary classes in physics. A vertical wire coil is surrounded by a spool of wire through which a large current can be passed upon throwing a switch. A metal ring which slips easily over this core is dropped around it from above. Immediately upon closing the circuit this ring is shot up into the air by the repulsive action of the electric current produced in the ring and the primary current in the coil. This scientific observation was developed by Professor Thomson into an alternating current repulsion motor which is nothing more nor less than our present repulsion induction motor.

In connection with this discovery the following quotation from the *Electrical World* of May 28, 1887 is of interest:

"It is, as yet, too early to assign to its proper place and limit the part which the alternating current will take in the electric arts. It has started on its career with most rapid strides, and it now only remains to devise means for its accurate measurement, regulation, and distribution. Certain it is that Professor Thomson's brilliant paper cannot fail to act as a powerful stimulus to those whose attention is now absorbed in the direction indicated, and the fruits of which will soon be noted. We hope that at a later meeting of the Institute Professor Thomson will give to the world his practical results, which he has only hinted at in the present paper."

In the field of electrical measuring instruments, he invented the "inclined-coil" instrument (1895), and the Thomson integrating wattmeter (1889). It is this latter meter which is now almost universally used for measuring amounts of electric cur-

rent used. In 1890 this instrument was exhibited in Paris and a prize of 10,000 francs for meters was divided between Thomson and Aron.

He next turned to the investigation of high-frequency phenomena. Already he had conceived the notion (1876), as I have mentioned, of tuning electric circuits, an operation fundamental to modern communication systems, and he had observed the propagation of electrical waves through space. In 1890 he patented a dynamo operating at frequencies 30 to 40 times greater than any previous machine. This led him to design high-frequency transformers. While working in this field he discovered (1893) a method of producing still higher frequency alternating current from a direct current arc, by shunting the arc with inductance and capacity, thus discovering the method which played such an important role in wireless transmission up until its virtual replacement by electronic tube devices. This interesting method of producing alternating currents was independently developed and applied to wireless telegraphy by Poulsen, and is therefore generally known as the Poulsen arc. During these high frequency investigations he made the important discovery that the insulating power of oils at these high frequencies is very much greater than at the ordinary low commercial frequencies, if this insulating power is measured in terms of the path at which a spark will pass.

After Röntgen announced his discovery of X-rays in 1895 Professor Thomson immediately began a series of experiments with them, the foundation for which had been laid by his previous experiments, beginning in 1891, on electric discharge through gases. In 1897 he made the first application of stereoscopic principles to X-rays, a great step forward in the medical use of X-rays for clinical purposes. He also made many improvements in the design of X-ray tubes, including the double-focus tube and a cooled-target tube. Along with these experiments he took a lively interest in the physiological effects of X-rays, going so far as to expose one of his fingers until a definite burn resulted.

Among his many other electrical inventions should be noted his resistance electric furnace patented in 1894, and a dynamo-

static machine (1900) by which it was possible to obtain high-frequency discharges suitable for vacuum-tube apparatus.

As I have already suggested, Professor Thomson did not confine his activity exclusively to electrical science. Jointly with his first colleague, Houston, he invented and patented (1881) a continuous cream separator, the precursor of the ultracentrifuge of today. In the field of steam engineering he secured a patent in 1903 on a "fluid pressure engine" of very high efficiency. This engine was later taken up by German engineers and reappeared under the name of the Stump Uniflow Engine.

Again in 1894 he devised a muffler for automobiles antedating the Maxim silencer and in many respects similar in principle. This muffler was based upon the sound scientific principle of dividing up an impulse or sound wave so that it should traverse a number of paths of unequal length in such a way that when these divided impulses all came together again, they would be out of phase and partially neutralize each other so as to take away or spread out the shock of the initial impulse. I can only mention further in the field of automotive engineering that Professor Thomson devised numerous types of gas and oil engines or improvements in their construction.

Professor Thomson began his career as a teacher of chemistry and he continued active in this area throughout his life. I have mentioned his early paper on the inhalation of gases and his suggestion that helium and nitrogen be used in deep sea diving. He early observed the transformation of ordinary carbon into graphite. In the nineties he published a series of papers on the uses of liquid air, and in 1906 published an article with the modern-sounding title "Alcohol and the Future of the Power Problem."<sup>10</sup> A patent granted to him in 1902 shows a method of forming hollow cylinders of quartz by the action of an arc drawn up through a bed of granular quartz. This was the beginning of his extended researches directed toward producing quartz disks for telescope mirrors. In a paper read before the American Philosophical Society in November, 1929, he described how he became convinced of the desirability of constructing these mirrors of quartz:

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<sup>10</sup> *Cassier's Magazine*, August 1906.

"It is now about thirty years since I made the first experiment, comparing a small slab of fused quartz or fused silica with a similar slab of glass, as a preliminary to further work. I formed on the surface of each of these a slightly concave surface, and then used well-known optical tests to show whether the figure was maintained under different conditions. The experiment was, naturally, imperfect, but I felt sure of the result. On having the two mounted so that I could have a distinct and clear image of a small artificial star, when used with an eye-piece as a telescope is used, I found that by instantaneous application of a moderate heat or a small flame on the back of the glass slab, the image went immediately all to pieces, as we may say; that is, it scattered; the definition was gone. A similar treatment of the quartz slab showed very little change, and not until the back of the quartz had become quite hot was there a semblance of the disturbance such as occurred with the glass. This experiment, modest as it was, convinced me that there was one material suitable for the making of astronomical reflectors that would avoid many of the difficulties of construction and operation inherent with the glass mirror telescopes."

He subsequently made mirrors for a small telescope at the Mount Wilson Observatory and undertook, at the request of the late George Ellery Hale, to prepare a huge fused quartz disk for the Mount Palomar 200-inch telescope.

"Through months and years of painstaking work, Dr. Thomson and his co-workers succeeded in producing larger and larger quartz disks for astronomical purposes, several of which have already been useful for the purposes intended. With every increase in size, however, new difficulties arose which he surmounted until at length quartz disks of five feet in diameter were actually secured. Here progress toward success appeared to be approaching an asymptote. While no difficulties ahead appeared unconquerable, time and cost began to impose harsh limits so that with many misgivings, it appeared expedient to revert to the more familiar and less expensive process of casting glass, if a sufficiently large disk for the 200-inch mirror were to be produced without undue postponement.

"It is now common knowledge that a large disk of Pyrex glass honeycombed to relieve excess weight was finally cast at the Corning Glass Works in March, 1934. Since Pyrex has a lower coefficient of expansion than the ordinary borosilicate glass, the finished product should show a considerable gain in performance in the direction to which Dr. Thomson devoted so

much of his energy, even though the result must unfortunately fall short of the high ideal he had set. It is perhaps fair to remark that he was very reluctant to forego further work in the fusing-of-quartz process for he was still confident that the mischievous obstacles which crept in with each increase in size were by no means insurmountable.”<sup>11</sup>

In the summer of 1858, when 5 years of age, Thomson had seen Donati's comet and in 1867 he witnessed spectacular meteor showers. These early observations prompted his abiding interest in astronomy. In 1878 he published an account of a method of grinding and polishing glass specula, and in 1899 he began the construction of a telescope for his private observatory, including the difficult task of making the optical parts for the 10-inch reflector. In later years he published nearly a score of papers on astronomical subjects ranging from discussions of zodiacal light to solar eclipses.

Still other scientific byways of Professor Thomson's interest were the earth sciences. He published on "The Nature and Origin of Volcanic Heat," and in his last appearance before the American Academy of Arts and Sciences in 1933, he read a paper on "The Krakatau Outbreak." The eruption of this volcano in Java occurred when he was a small boy in Philadelphia, and had incited the curiosity which he always exhibited. He had watched for evidences, in the brilliant sunsets, of the volcanic ash in the upper atmosphere and had, I am informed, recorded his observations. At a much later date he hired as a research assistant the sole survivor of the catastrophe and induced him to record his personal observations of the event. In his paper before the Academy he reported on this record, upon the history of the eruption and upon his own boyhood observations of its effects. Coupled with these more formal observations were his love of mountain climbing and his activities as an amateur naturalist.

With all this intensive activity, Professor Thomson lived a rich family life. He was married on May 1, 1884, to Mary L.,

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<sup>11</sup> "The Astronomical Contributions of Elihu Thomson," a paper read by Harlan T. Stetson at the meeting in commemoration of the life and work of Professor Thomson, February 16, 1939. In the files of the American Philosophical Society, Philadelphia.

daughter of Charles Peck of New Britain, Conn., and of this union there were four sons, Stuart, Roland D., Malcolm and Donald T. In 1916 Mrs. Thomson died, and on January 4, 1923, he was married to Clarissa, daughter of Theodore F. Hovey of Boston. He had a charming home at 22 Monument Avenue, Swampscott, Mass., two of the striking features of which were his excellent shop and observatory. One of his most notable characteristics was his deep and understanding interest in children.

During his life he received wide recognition for his achievements. His honorary degrees included an A.M. from Yale in 1890, a Ph.D. from Tufts College in 1894, a D.Sc. from Harvard University in 1909, and from the University of Manchester, England, in 1924, and the LL.D. from the University of Pennsylvania in the same year.

Among the many medals and prizes he received were: the John Scott Legacy Medal and Premium of the Franklin Institute; the Rumford Medal, 1902, of the American Academy of Arts and Sciences; the Hughes Medal, 1916, of the Royal Society of Great Britain; the Edison Medal, 1910, of the American Institute of Electrical Engineers; the Elliott Cresson Medal, 1912, of the Franklin Institute; the John Fritz Medal, 1916, given by the founder of engineering societies; the Lord Kelvin Medal, 1924, of the English engineering societies; the Franklin Medal, 1925, of the Franklin Institute; the Faraday Medal, 1927, of the Institution of Electrical Engineers, London; the Grand Prix at the Paris Expositions of 1889 and 1900, and the Grashof Medal, 1935, of the Verein Deutscher Ingenieure. He was made Officier et Chevalier de la Legion d'Honneur in 1889.

Of his many affiliations with societies the following may be noted: Fellow of the American Association for the Advancement of Science, American Institute of Electrical Engineers (President, 1889-90), American Chemical Society, American Philosophical Society, National Academy of Sciences, American Academy of Arts and Sciences, Institution of Civil Engineers of Great Britain, and honorary member of the Franklin Institute and Institution of Electrical Engineers of Great Britain. Among the important offices he held was the presidency, succeeding

Lord Kelvin in 1908, of the International Electrotechnical Commission.

Behind all his astonishingly varied interests, stood a man who had complete faith in the efficacy of the scientific method, and who in all his activities, vocational and avocational, was a shining exemplar of the scientific spirit. Something of his own view of his methods was incorporated in an address delivered by him in 1899 as vice-president and chairman of the physics section of the American Association for the Advancement of Science, in which he said :

“The development in the field of research by experiment is like the opening of a mine, which, as it deepens and widens, continually yields new treasure but with increased difficulty, except when a rich vein is struck and worked for a time. In general however, as the work progresses there will be needed closer application and more refined methods. In most fields of research the investigator must be ready to guide the trained mechanic and be able himself to administer those finishing touches which often mark the difference between success and failure. There must be in his mental equipment that clear comprehension of the proper adjustment of means to ends which is of such great value in work in new fields. He must also learn to render available to science the resources of the larger workshops and industrial establishments. . . . Scientific facts are of little value in themselves. Their significance has a bearing upon other facts, enabling us to generalize and to discover principles, just as the accurate measurements of the position of a star may be without value in itself, but in relation to other similar measurements of other stars may become the means of discovering their proper motion. We refine our instruments, we render more trustworthy our means of observation, we extend our range of experimental inquiry and thus lay the foundation for future work with the full knowledge that although our researches cannot extend beyond certain limits, the field itself is even within those limits inexhaustible.”

Observation and experimental inquiry were his chief reliances ; he apparently did not resort to the mathematical or analytical methods that most scientists and engineers use who tackle problems as complex as he solved. He was not, like Steinmetz, a gifted mathematician ; he seemingly did not need to employ

mathematical analysis because his teeming mind leapt to correct conclusions without it.

His powers of observation he carried into every walk of life, and no one could be with him for ten minutes without being impressed and stimulated by his perception and by his wide-ranging knowledge of natural phenomena. He could best be described by saying that he was a brilliant natural philosopher who was held in equally high esteem by practical engineers and by academic scientists.

Perhaps the most eloquent testimony to his scientific contributions may be found in the widespread appreciation today of the value of research in industry. Professor Thomson was one of the first in America to recognize the importance of research, both fundamental and practical, to our industrial progress. This was a contribution that may transcend any of his scientific discoveries.

I have spoken of his devotion to education. His long association with the Massachusetts Institute of Technology affords a specific example. He became a lecturer in electrical engineering at this institution in 1894, and from then until his death he maintained with it the closest sort of relationship. He was elected a life member of the corporation in 1898, was acting president from 1920 to 1923, and for many years was a member of the executive committee of the corporation. He likewise served Harvard University as a lecturer and as a member of several of its visiting committees.

In other ways he never ceased to teach. His friend, Dr. Richard C. Maclaurin, President of M. I. T. from 1909 to 1920, observed:

"Throughout his life he has not only done great things himself but shown an intense desire to help all who are struggling earnestly with scientific problems. He has proved an inspiration to an ever-widening circle of engineers and others who have intrusted him with their secrets and sought his help in overcoming their difficulties. They have done this, knowing that they had only to ask in order to get the full benefit of his imagination and his power, and that they need have no misgivings that he would take any advantage of their confidence or any credit for their work, for he has no touch of selfishness."

From my own association with him I can validate Dr. MacLaurin's tribute. He combined in a most remarkable way the constructive powers of the inventor, the intuition and imagination of the great scientist, and the kindly balance of the ideal philosopher, teacher and friend. Perhaps no inventor save Edison has brought more renown to our country or contributed so much to its recent material progress. His life encompassed the development of the electrical industry, and he will long be remembered as one of those who brilliantly extended and applied the primary discoveries of Faraday and the other pioneers in the science of electricity.

He died on March 13, 1937, in his eighty-fourth year.

ELIHU THOMSON—COMPTON

Key to Abbreviations Used in Bibliography

Elec. Eng.—Electrical Engineering  
Elec. Rec.—Electrical Record  
Elec. Rev.—Electrical Review  
Elec. World—Electrical World  
Electr. & Elec. Eng.—Electrician and Electrical Engineer  
Eng. Mag.—Engineering Magazine  
Eng. Mech.—English Mechanic  
Gen. Elec. Rev.—General Electric Review  
Journ. Franklin Inst.—Journal Franklin Institute  
Phila. Med. Times—Philadelphia Medical Times  
Trans. Amer. Electro-Therapeutic Assn.—Transactions American Electro-Therapeutic Association

SELECTED LIST OF THE PUBLICATIONS \*  
OF ELIHU THOMSON

1873

On the Inhalation of Nitrous Oxide, Nitrogen, Hydrogen and Other Gases and Gaseous Mixtures. Phila. Med. Times, November 15.

1876

Electrical Phenomena. Journ. Franklin Inst., April, Vol. 101, pp. 270-4.

1877

Method of Grinding Glass Specula. Eng. Mech., XXVII, 1878.

1878

Report of Committee on Dynamo Electric Machines. Journ. Franklin Inst., May and June 1878, Vol. 105, pp. 289-303, Vol. 105, pp. 361-77.  
A New System of Electric Lighting, and a New Form of Electric Lamp. Journ. Franklin Inst., October, Vol. 106, pp. 251-3.

1886

Welding by Electricity. Electrician (London), September 17, Vol. 17, p. 392.

Electrical Welding. Elec. World, December 25, Vol. 8, pp. 307-9.

1887

Electric Welding. Journ. Franklin Inst., May, Vol. 123, pp. 357-70.  
Novel Phenomena of Alternating Currents. Electr. and Elec. Eng., June, Vol. 6, pp. 211-15.

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\* The American Philosophical Society has in its files in Philadelphia an extensive list of papers by and about Professor Thomson.

1888

- Electric Wave and Phase Indicator for Alternating and Undulatory Currents. *Elec. World*, January 28, Vol. 11, pp. 39-40.  
 Safety Devices with Transformers. *Electrician* (London), March 16, Vol. 20, p. 520.  
 Phenomena of Magnetic Propagation. *Elec. World*, October 27, Vol. 12, pp. 220-2.

1889

- Note on the Recalescence of Iron. *Elec. Rev.*, April 26, Vol. 24, p. 471.  
 On Recalescence in Steel and Its Relation to Expansion, Magnetization and Hardening. *Elec. World*, May 25, Vol. 13, p. 295.  
 Magnetism in Its Relation to Induced E.M.F. and Current. *Elec. World*, June 1, Vol. 13, pp. 315-16.

1890

- Safety and Safety Devices in Electric Installations. *Elec. Eng.*, March, Vol. 9, pp. 95-8.  
 Phenomena of Alternating Current Induction. *Elec. Eng.*, April 9, Vol. 9, pp. 212-14.  
 Loss by Hysteresis. *Elec. Eng.*, April 30, Vol. 9, pp. 295-6.

1891

- Induction of Alternating Currents and Induction Coils. *Journ. Franklin Inst.*, August, Vol. 132, pp. 81-101.  
 Physiological Effects of Alternating Currents of High Frequency. *Elec. Eng.*, March 11, Vol. 11, pp. 304-5.  
 Notes on Alternating Currents at Very High Frequency. *Elec. Eng.*, March 11, Vol. 11, p. 300.  
 The Electric Arc and Its Use in Lighting. *Elec. World*, February 28, Vol. 7, pp. 166-70.  
 Phenomena of Alternating Currents of Very High Frequency. *Elec. World*, April 4, Vol. 17, p. 254.  
 Oil Insulation for High Tension Transformers. *Elec. Eng.*, April 15, Vol. 11, p. 440.  
 Discussion on the Phenomena of Currents of High Frequency. *Elec. Eng.*, April 22, Vol. 11, p. 474.  
 Practical Aspects of Electrical Welding. *Elec. World*, June 6, Vol. 17, pp. 421-2.  
 Constant Current Transformers. *Elec. World*, July 4, Vol. 18, p. 9.

1892

- Induction by High Potential Discharge. *Elec. Rev.* March 18, Vol. 30, pp. 348-50.  
 Further Experiments with Condenser, Spark and Air Jet. *Elec. Rev.*, April 15, Vol. 30, p. 493-5.  
 High Potential Transmission. *Elec. World*, March 26, Vol. 19, pp. 214-15.

ELIHU THOMSON—COMPTON

1893

Single Phase Alternating Motors. Elec. Eng., March 15, Vol. 15, pp. 272-3.

1894

Effects of High Frequency Electrical Discharges Passed Through the Body. Trans. Amer. Electro-Therapeutic Assn., September 27.

How the Carbon Brush Came into Use. Elec. Eng., October 3, Vol. 18, p. 265.

1896

Stereoscopic Roentgen Pictures. Elec. Eng., March 11, Vol. 21, p. 256.

Dielectric Strength of Oils in Alternating Potentials. Elec. Eng., February 12, Vol. 21, pp. 160-61.

A Proposed Standard Tube for Producing Roentgen Rays, Double Focus Tube. Elec. World, April 18, Vol. 27, p. 426.

Diffusion and Opalescence with Roentgen Rays. Elec. World, April 25, Vol. 27, p. 452.

Roentgen Rays Act Strongly on the Tissues. Elec. Eng., November 5, Vol. 22, p. 534.

1897

Roentgen Ray Burns. Elec. Eng., April 14, Vol. 23, p. 400.

Roentgen Ray Diffusion Phenomena. Elec. Eng., June 23, Vol. 23, pp. 699-700.

1899

Possibilities of Liquid Air. Eng. Mag., May, Vol. 17, pp. 197-205.

The Field of Experimental Research. Science, August 25, Vol. 10, pp. 236-45.

Apparatus for Obtaining High Frequencies and Pressures. Elec. World, October 14, Vol. 34, pp. 576-8.

1906

Alcohol and the Future of the Power Problem. Cassier's Magazine, August, Vol. 30, pp. 310-12.

The Nature and Origin of Volcanic Heats. Science, August 10, Vol. 24, pp. 161-6.

1909

The Electric Arc and Recent Advances in Arc Lighting. Elec. Rec., June.

1922

Silica Glass or Fused Quartz. Gen. Elec. Rev., February 1923, Vol. 26, pp. 68-74.

1927

Helium in Deep Sea Diving. Science, January 14, Vol. 65, pp. 36-8.

A Suggestion or Hypothesis Concerning the Zodiacal Light. Science, October 21, Vol. 66, pp. 376-8.

The Pioneer Investigations on Dynamo Machines Fifty Years Ago. Journ. Franklin Inst., July 1928, Vol. 206, pp. 17-25.

## The United States Patents of Elihu Thomson

A list of Professor Thomson's patents is more significant as an index of his important work than is a bibliography of his publications. Consequently the total list of nearly 700 is presented here as obtained from J. A. McManus, Professor Thomson's secretary. I have ventured to star those which seem to me to be the most important.—*The Author.*

<i>No.</i>	<i>Title</i>	<i>Date</i>
177,124	—Street Railway Rail Fastener.....	May 9, 1876
183,031	—Relays and Sounders.....	Oct. 10, 1876
*219,157	—Dynamo Electric Machine.....	Sept. 2, 1879
220,287	—Regulator for Electric Lamps.....	Oct. 7, 1879
220,507	—Galvanic Battery Cell.....	Oct. 14, 1879
220,508	—Regulator for Electric Lamp.....	Oct. 14, 1879
220,948	—Proc. & App. for Storage of Electricity.....	Oct. 28, 1879
*223,557	—Dynamo Electric Machine.....	Jan. 13, 1880
223,646	—Regulator for Electric Lamps.....	Jan. 20, 1880
*223,658	—Arm. & Com. for Mag. El. Machines.....	Jan. 20, 1880
*223,659	—Aut. Adj. for Com. Brushes on Mag. Electric Machines.....	Jan. 20, 1880
232,910	—Dynamo Electric Machine.....	Oct. 5, 1880
*233,047	—Dynamo Electric Machine.....	Oct. 5, 1880
*238,315	—Cur. Reg. for Dynamo Electric Machines....	Mar. 1, 1881
*239,659	—Centrifugal Creamer.....	Apr. 5, 1881
*242,488	—Com's for Dynamo El. Machines.....	June 7, 1881
250,175	—Electro Magnetic Device.....	Nov. 29, 1881
250,463	—Electric Lamp.....	Dec. 6, 1881
*253,958	—Electric Lamp.....	Feb. 21, 1882
255,824	—System of Electric Distribution.....	Apr. 4, 1882
256,605	—Electric Lamp.....	Apr. 18, 1882
258,684	—Electric Arc Lamp.....	May 30, 1882
261,067	—Electric Arc Lamp.....	July 11, 1882
261,790	—Electric Arc Lamp.....	July 25, 1882
*265,936	—Means for Preventing Flashing between Electric Conductors.....	Oct. 10, 1882
265,937	—Reg. for Dynamo Electric Machines.....	Oct. 10, 1882
265,993	—Electric Arc Light.....	Oct. 17, 1882
269,605	—Dynamo Electric Machine.....	Dec. 26, 1882
*269,606	—Dynamo Electric Machine (Reg. for).....	Dec. 26, 1882
*271,947	—Com. for Dynamo Electric Machines.....	Feb. 6, 1883
*271,948	—Electric Current Regulator.....	Feb. 6, 1883
272,353	—Electro-Magnetic Retarding Device.....	Feb. 13, 1883
*272,920	—Electric Arc Lamp.....	Feb. 27, 1883
*273,406	—Air Blast Attachment for Commutators of Dynamo Elec. Mchs.....	Mar. 6, 1883
274,413	—Electric Arc Lamp.....	Mar. 20, 1883
275,289	—Safety Self-Closing Shunt Switch for Elec. Light Circuits.....	Apr. 3, 1883
275,290	—Safety Self-Closing Shunt Switch for Elec. Light Circuits.....	Apr. 3, 1883
281,416	—Dynamo Electric Machine.....	July 17, 1883
*283,167	—Electric Com. or Switch.....	Aug. 14, 1883
283,168	—Electric Lamp.....	Aug. 14, 1883
283,437	—Electric Lamp.....	Aug. 21, 1883
289,580	—Safety Device for Electric Arc Lamps.....	Dec. 4, 1883
294,094	—Dynamo Electric Machine.....	Feb. 26, 1884
294,095	—El. Power Distributing System.....	Feb. 26, 1884

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
295,836—	Double Carbon Arc Lamp.....	Mar. 25, 1884
296,569—	Dynamo Electric Machine.....	Apr. 8, 1884
296,799—	Dynamo Electric Machine.....	Apr. 15, 1884
297,194—	Electric Arc Lamp.....	Apr. 22, 1884
297,195—	Electric Arc Lamp.....	Apr. 22, 1884
297,196—	Electric Arc Lamp.....	Apr. 22, 1884
297,197—	Electric Arc Lamp.....	Apr. 22, 1884
297,198—	Electric Arc Lamp.....	Apr. 22, 1884
297,199—	Electric Arc Lamp.....	Apr. 22, 1884
297,200—	Electric Arc Lamp.....	Apr. 22, 1884
297,201—	Electric Arc Lamp.....	Apr. 22, 1884
302,960—	Electric Lamp.....	Aug. 5, 1884
302,961—	Focusing Electric Arc Lamp.....	Aug. 5, 1884
302,962—	Electric Arc Lamp.....	Aug. 5, 1884
302,963—	Reg. for Dynamo Electric Machines.....	Aug. 5, 1884
303,762—	Electric Arc Lamp.....	Aug. 19, 1884
303,898—	Electric Lamp Mechanism.....	Aug. 19, 1884
*305,413—	Electric Lamp Mechanism.....	Sept. 16, 1884
306,118—	Electric Arc Lamp.....	Oct. 7, 1884
306,119—	Electric Arc Lamp.....	Oct. 7, 1884
307,818—	Aut. Cut-out for El. App.....	Nov. 11, 1884
307,819—	Cut-out for Electric Arc Lamps.....	Nov. 11, 1884
320,017—	Cut-out for Electric Circuits.....	June 16, 1885
320,018—	Electric Lamp.....	June 16, 1885
*321,461—	Electric Lighting System.....	July 7, 1885
*321,463—	Electric Switch.....	July 7, 1885
*321,464—	Lightning Arrester.....	July 7, 1885
*322,138—	System of Electrical Distribution.....	July 14, 1885
322,139—	System of Electrical Distribution.....	July 14, 1885
*323,975—	Dynamo El. or El. Dynamic Machine.....	Aug. 11, 1885
323,976—	Aut. Com. Adj. for Dynamo Electric Machine.....	Aug. 11, 1885
324,501—	Reg. for Dynamo Electric Machines.....	Aug. 18, 1885
324,502—	Electric Arc Lamp.....	Aug. 18, 1885
*327,039—	Safety Device for Electric Circuits.....	Sept. 29, 1885
*333,573—	Dynamo Electric Machine.....	Jan. 5, 1886
335,158—	Incandescent Electric Lamp.....	Feb. 2, 1886
*335,159—	System of Electric Distribution.....	Feb. 2, 1886
335,160—	Incandescent Electric Lamp.....	Feb. 2, 1886
335,547—	Electric Motor.....	Feb. 2, 1886
335,548—	Switch or Turn-off for Electric Circuits.....	Feb. 2, 1886
338,208—	Aut. Cut-out for Electric Lamps.....	Mar. 16, 1886
*339,079—	Reg. for Dynamo Electric Machines.....	Mar. 30, 1886
339,714—	Electric Switch.....	Apr. 13, 1886
344,692—	Holder for Incandescent Lamps.....	June 29, 1886
345,334—	Electro Magnetic Motor.....	July 13, 1886
345,335—	Socket for Incandescent Lamps.....	July 13, 1886
345,336—	Commutator Brush.....	July 13, 1886
*347,140—	Apparatus for Electric Welding.....	Aug. 10, 1886
*347,141—	Apparatus for Electric Welding.....	Aug. 10, 1886
*347,142—	Electric Welding.....	Aug. 10, 1886
349,912—	Compound Wound Dynamo Electric Machine.....	Sept. 28, 1886
350,955—	Cut-out App. for Electric Lamps.....	Oct. 19, 1886
350,956—	Aut. Compensator for Magnets.....	Oct. 19, 1886
350,957—	Electro Magnet.....	Oct. 19, 1886
350,958—	Distribution of Electric Currents.....	Oct. 19, 1886
353,179—	Regulator for Electric Currents.....	Nov. 23, 1886
353,180—	Reg. for Dynamo El. Machines, Motors, etc. . .	Nov. 23, 1886

<i>No.</i>	<i>Title</i>	<i>Date</i>
354,272—	App. for the Distribution of Electricity by Means of Secondary Batteries.....	Dec. 14, 1886
354,273—	Reg. for Dynamo Electric Machines.....	Dec. 14, 1886
354,274—	Induction Coil.....	Dec. 14, 1886
356,902—	Arm. for Dynamo Electric Machines.....	Feb. 1, 1887
356,903—	Electric Arc Lamp.....	Feb. 1, 1887
358,131—	Pump for Producing High Vacua.....	Feb. 22, 1887
360,122—	System of Electric Distribution.....	Mar. 29, 1887
360,123—	Electro Magnetic Cut-off.....	Mar. 29, 1887
360,124—	Automatic Cut-off.....	Mar. 29, 1887
*360,125—	System of Electric Distribution.....	Mar. 29, 1887
363,183—	Electric Switch.....	May 17, 1887
363,184—	Automatic Commutator Adjuster.....	May 17, 1887
*363,185—	Alternating Current Electric Motor.....	May 17, 1887
*363,186—	Alternating Current Motor Device.....	May 17, 1887
365,553—	Electric Arc Lamp.....	June 28, 1887
367,469—	System of Electric Distribution.....	Aug. 2, 1887
367,470—	Reg. for Dynamo Electric Machines and Motors.....	Aug. 2, 1887
367,471—	Coupling Compound Wound Dynamo Machines.....	Aug. 2, 1887
367,866—	Coupling Dynamo Electric Machines.....	Aug. 9, 1887
369,754—	Dynamo Electric Machine or Motor.....	Sept. 13, 1887
*370,572—	Electric Arc Lamp.....	Sept. 27, 1887
370,573—	Re. Device for Alternating Current Circuits.....	Sept. 27, 1887
372,501—	System of Electric Distribution for Alternating Currents.....	Nov. 1, 1887
373,108—	Electric Motor.....	Nov. 15, 1887
375,022—	Electric Welding.....	Dec. 20, 1887
375,784—	Apparatus for Electric Welding.....	Jan. 3, 1888
376,120—	Dynamo Electric Machine or Motor.....	Jan. 10, 1888
377,217—	Alternating Current Motor and Regulating Device.....	Jan. 31, 1888
381,441—	Electric Meter.....	Apr. 17, 1888
381,442—	Electro-Mechanical Device.....	Apr. 17, 1888
381,443—	Electric Meter.....	Apr. 17, 1888
382,335—	Alternating Current Dynamo Electric Machine.....	May 8, 1888
382,336—	Alternating Current Regulator.....	May 8, 1888
385,022—	Apparatus for Electric Welding.....	June 26, 1888
385,384—	Joining Pipes by Electricity.....	July 3, 1888
385,385—	Electrically Welding Chains and Links.....	July 3, 1888
385,386—	Direct Electric Welding Machine.....	July 3, 1888
385,647—	Electric Meter.....	July 3, 1888
386,441—	Apparatus for Electric Welding.....	July 17, 1888
387,123—	Flue for Electric Transformers.....	July 31, 1888
389,265—	Thermal Device for Varying Electric Resistance of Currents.....	Sept. 11, 1888
389,779—	Direct Welding Dynamo Electric Machine.....	Sept. 18, 1888
390,318—	Alternating Current Dynamo.....	Oct. 2, 1888
391,437—	Electrical Potential Differentiator.....	Oct. 23, 1888
392,765—	Dynamo Electric Machine.....	Nov. 13, 1888
393,040—	Electric Light Pole.....	Nov. 20, 1888
394,892—	Portable Electric Welding Apparatus.....	Dec. 18, 1888
395,018—	Electric Meter.....	Dec. 25, 1888
396,009—	Forming, Brazing and Welding of Metals by Electricity.....	Jan. 8, 1889

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
396,010—	Electric Forging.....	Jan. 8, 1889
396,011—	Electric Welding.....	Jan. 8, 1889
396,012—	Apparatus for Electric Welding and Work- ing Metals.....	Jan. 8, 1889
396,013—	Electric Pipe Joining and Pipe Work.....	Jan. 8, 1889
396,014—	Electric Metal Working.....	Jan. 8, 1889
396,015—	Electric Riveting.....	Jan. 8, 1889
397,616—	Regulating and Motive Device for Alter- nating Currents.....	Feb. 12, 1889
398,912—	Manufacturing Screws and Bolts by Elec- tricity.....	Mar. 5, 1889
398,913—	Electric Welding Machine.....	Mar. 5, 1889
398,914—	Electric Metal Working and Welding Machine.....	Mar. 5, 1889
399,800—	Dynamo Electric Machine.....	Mar. 19, 1889
399,801—	Alternating Current Inductor.....	Mar. 19, 1889
*400,515—	Apparatus for Regulating Current or Po- tential in Secondary of Transformers.....	Apr. 2, 1889
*400,516—	Method of Regulating Current or Potential in Secondary of Transformers.....	Apr. 9, 1889
400,971—	Alternating Current Electric Motor.....	Apr. 9, 1889
400,972—	Induction Coil and Self-Inductive Apparatus.....	Apr. 9, 1889
400,973—	Armature for Dynamo Electric Machines.....	Apr. 9, 1889
*401,085—	Lightning Arrester.....	Apr. 9, 1889
401,608—	Distribution of Electric Currents.....	Apr. 16, 1889
401,803—	Electric Meter.....	Apr. 23, 1889
403,157—	Method of Electric Welding and Shaping of Metals.....	May 14, 1889
403,707—	Process of Electric Soldering, Brazing & Weld- ing.....	May 21, 1889
403,708—	Method of Electric Welding and Brazing.....	May 21, 1889
406,010—	Electric Meter.....	June 25, 1889
407,844—	Alternating Current Electric Motor.....	July 30, 1889
409,714—	Induction Coil.....	Aug. 27, 1889
410,468—	Reactive and Induction Coil.....	Sept. 3, 1889
413,292—	Electric Measuring Instrument.....	Oct. 22, 1889
413,293—	System of Electrical Distribution.....	Oct. 22, 1889
413,294—	Conduit for Electrical Railways.....	Oct. 22, 1889
414,266—	Iron-Cased Induction Coil for Alternating Current Transfer.....	Nov. 5, 1889
415,305—	Electric Welding Clamp.....	Nov. 19, 1889
415,747—	Electric Meter.....	Nov. 26, 1889
415,748—	Electric Meter.....	Nov. 26, 1889
415,749—	Electric Transformers.....	Nov. 26, 1889
416,350—	Electric Meters.....	Dec. 3, 1889
416,762—	Induction Coil, Transformer, etc.....	Dec. 10, 1889
418,198—	Method of Making Collars on Axles by Elec- tricity.....	Dec. 31, 1889
418,249—	Lightning Arrester.....	Dec. 31, 1889
420,306—	Electric Transformer.....	Jan. 28, 1890
421,207—	Cut-out for Incandescent Lamps.....	Feb. 11, 1890
421,208—	System of Distribution for Alternating Cur- rents.....	Feb. 11, 1890
422,550—	Compound Insulating Layer for Electric Coils.....	Mar. 4, 1890
422,999—	Field Magnet for Dynamos.....	Mar. 11, 1890
423,965—	Electric Valve Controller.....	Mar. 25, 1890
423,966—	Method of Electric Soldering, Cementing, etc.....	Mar. 25, 1890

<i>No.</i>	<i>Title</i>	<i>Date</i>
423,967	Apparatus for Electric Soldering and Cementing.....	Mar. 25, 1890
425,470	Distribution of Electric Currents.....	Apr. 15, 1890
425,588	Cut-out.....	Apr. 15, 1890
425,640	Guard for Electric Railway Trolleys.....	Apr. 15, 1890
426,082	Safety Connection.....	Apr. 22, 1890
426,348	Dynamo Electric Machine.....	Apr. 22, 1890
428,647	Turn-off for Alternating Current Circuits....	May 27, 1890
*428,648	Casing for Induction Coils.....	May 27, 1890
428,649	Electric Meter.....	May 27, 1890
*428,650	Alternating Current Magnetic Device.....	May 27, 1890
428,651	System of Electrical Distribution.....	May 27, 1890
428,652	Incandescent Lamp Socket.....	May 27, 1890
428,653	Guard Wire Protector and Lightning Arrester for Electric Railways.....	May 27, 1890
428,704	Electric Switch.....	May 27, 1890
428,705	Regulator for Dynamo Electric Machines....	May 27, 1890
430,326	Electro-Magnetic Cut-out for Electric Lamps..	June 17, 1890
430,327	Regulator for Dynamo Electric Machines....	June 17, 1890
430,328	Alternating Current Motor.....	June 17, 1890
430,357	Electric Arc Lamp.....	June 17, 1890
431,414	Electric Railway Conductor.....	July 1, 1890
432,581	Frog for Overhead Wires.....	July 22, 1890
432,651	Method of Working Metals by Electricity....	July 22, 1890
432,652	Welding or other Dynamo.....	July 22, 1890
432,653	Method of Welding Pipes by Electricity.....	July 22, 1890
432,654	Electric Meter.....	July 22, 1890
*432,655	Dynamo Electric Machine.....	July 22, 1890
432,656	Manufacture of Bands, Rings, etc. by Elec- tricity.....	July 22, 1890
434,488	Electric Power Transmission.....	Aug. 19, 1890
434,489	Electric Power System.....	Aug. 19, 1890
434,530	Process of and Apparatus for Forming and Welding Metals by Electricity.....	Aug. 19, 1890
434,531	Induction Discharge Protector for Welding Apparatus.....	Aug. 19, 1890
434,532	Process of Electric Welding.....	Aug. 19, 1890
434,961	Section Insulator for Overhead Electric Con- ductors.....	Aug. 26, 1890
435,870	Suspending Device for Overhead Electric Conductors.....	Sept. 2, 1890
438,204	Electric Motor.....	Oct. 14, 1890
438,656	Electric Motor.....	Oct. 21, 1890
438,657	Process of Electric Welding.....	Oct. 21, 1890
438,658	Electric Welding of Pipes.....	Oct. 21, 1890
440,662	Electric Arc Lamp.....	Nov. 18, 1890
440,663	Electric Arc Lamp.....	Nov. 18, 1890
440,664	Method of Electric Welding.....	Nov. 18, 1890
440,665	Trolley Arm for Electric Railways.....	Nov. 18, 1890
*444,678	Lightning Arrester.....	Jan. 13, 1891
444,925	Electric Arc Lamp.....	Jan. 20, 1891
444,926	Method of Electric Welding.....	Jan. 20, 1891
444,927	Method of Electric Welding.....	Jan. 20, 1891
444,928	Method of Electric Welding.....	Jan. 20, 1891
444,929	Incandescent Electric Lamp.....	Jan. 20, 1891
444,930	System of Distributing and Metering Electric Energy.....	Jan. 20, 1891

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
444,931—	Electric Meter . . . . .	Jan. 20, 1891
444,946—	Method of Electric Welding . . . . .	Jan. 20, 1891
446,483—	Electric Railway Conductor . . . . .	Feb. 17, 1891
447,383—	Electric Arc Lamp . . . . .	Mar. 3, 1891
447,384—	Dynamo Electric Motor or Generator . . . . .	Mar. 3, 1891
448,279—	Electric Lighting System . . . . .	Mar. 17, 1891
448,280—	Electric Meter . . . . .	Mar. 17, 1891
*448,894—	Electric Meter . . . . .	Mar. 24, 1891
449,356—	Manufacturing Chains by Electric Welding Process . . . . .	Mar. 31, 1891
449,357—	Burr Preventer for Electric Welding Machines . . . . .	Mar. 31, 1891
449,715—	Electric Arc Lamp . . . . .	Apr. 7, 1891
449,836—	Method of Electric Welding . . . . .	Apr. 7, 1891
450,687—	Railroad Gate Crossing for Overhead Lines . . . . .	Apr. 21, 1891
451,345—	Method of Electric Welding . . . . .	Apr. 28, 1891
452,951—	Armature for Dynamo Electric Machines or Motors . . . . .	May 26, 1891
454,090—	Transformer . . . . .	June 16, 1891
454,671—	Lightning Arrester . . . . .	June 23, 1891
454,672—	Lightning Arrester . . . . .	June 23, 1891
*454,673—	Lightning Arrester . . . . .	June 23, 1891
454,782—	Lamp Cut-Out and System . . . . .	June 23, 1891
454,890—	Apparatus for Removing Inductive Effects from Electric Lines . . . . .	June 30, 1891
455,420—	Method of Electric Welding . . . . .	July 7, 1891
455,421—	Securing Metal Bands on Wooden or other Articles . . . . .	July 7, 1891
455,905—	Automatic Hammer . . . . .	July 14, 1891
456,172—	Method of Measuring Electric Currents . . . . .	July 21, 1891
457,036—	Electric Motor for Street Cars . . . . .	Aug. 4, 1891
458,025—	Electric Arc Lamp . . . . .	Aug. 18, 1891
*458,115—	Method of Electric Bending and Straighten- ing . . . . .	Aug. 18, 1891
458,646—	Electric Motor . . . . .	Sept. 1, 1891
*459,422—	Dynamo Electric Machine and Motor . . . . .	Sept. 15, 1891
459,423—	System of Electrical Distribution . . . . .	Sept. 15, 1891
460,765—	Composition for Insulating Material . . . . .	Oct. 6, 1891
461,144—	Electric Arc Lamp . . . . .	Oct. 13, 1891
461,526—	Adjustable Transformer . . . . .	Oct. 20, 1891
461,856—	Mode of Making Tools . . . . .	Oct. 27, 1891
*462,338—	Incandescent Lamp . . . . .	Nov. 3, 1891
462,339—	Incandescent Lamp . . . . .	Nov. 3, 1891
462,973—	Brush Holder for Dynamo Electric Machines . . . . .	Nov. 10, 1891
463,671—	Armature Core for Dynamo Electric Machines . . . . .	Nov. 24, 1891
463,761—	Section Insulator and Lightning Arrester for Electric Railroads . . . . .	Nov. 24, 1891
463,762—	Electric Arc Interrupter . . . . .	Nov. 24, 1891
464,595—	Lightning Arrester . . . . .	Dec. 8, 1891
*465,078—	Method of Controlling Alternating Current Induction . . . . .	Dec. 15, 1891
467,318—	Commutator for Dynamo Electric Machines and Motor . . . . .	Jan. 19, 1892
468,119—	Electric Switch . . . . .	Feb. 2, 1892
468,120—	Method of and Means for Interrupting Elec- tric Currents . . . . .	Feb. 2, 1892
468,121—	Dynamo Electric Machine . . . . .	Feb. 2, 1892
468,122—	System of Electric Distribution . . . . .	Feb. 2, 1892

<i>No.</i>	<i>Title</i>	<i>Date</i>
468,123	—System of Electrical Distribution.....	Feb. 2, 1892
468,497	—Lightning Arrester.....	Feb. 9, 1892
470,221	—Electric Railway.....	Mar. 2, 1892
470,721	—Lightning Arrester.....	Mar. 15, 1892
*471,155	—Alternating Current Motor.....	Mar. 22, 1892
476,330	—Safety Device for Electric Motors.....	June 7, 1892
476,331	—Dynamo Electric Machine.....	June 7, 1892
476,967	—Manufacture of Axes.....	June 14, 1892
478,145	—Electric Arc Lamp.....	July 5, 1892
478,722	—Distribution of Electric Currents.....	July 12, 1892
480,392	—Method of Electric Soldering.....	Aug. 9, 1892
481,878	—System of Telephony.....	Aug. 30, 1892
482,209	—Induction Coil for Electric Meters.....	Sept. 6, 1892
482,397	—Ventilating Armatures for Dynamo Electric Machines.....	Sept. 13, 1892
483,700	—Armatures for Dynamos and Motors.....	Oct. 4, 1892
485,239	—Regulator for Dynamo Electric Machines....	Nov. 1, 1892
485,669	—Continuous Current Transformer.....	Nov. 8, 1892
486,916	—Electrical Transformer.....	Nov. 29, 1892
487,302	—Method of Electric Welding.....	Dec. 6, 1892
488,585	—Electric Arc Lamp.....	Dec. 27, 1892
489,046	—Electric Arc Lamp.....	Jan. 3, 1893
490,178	—Electric Circuit Breaker.....	Jan. 17, 1893
490,376	—Armature for Dynamo Electric Machines or Motor.....	Jan. 24, 1893
490,839	—Thermal Circuit Closer.....	Jan. 31, 1893
493,313	—Dynamo Electric Machine.....	Mar. 14, 1893
*493,314	—Lightning Arrester.....	Mar. 14, 1893
493,739	—Electric Arc Lamp.....	Mar. 21, 1893
495,071	—Compressed Air Apparatus.....	Apr. 11, 1893
495,714	—Lightning Arrester.....	Apr. 18, 1893
*495,853	—Lightning Arrester.....	Apr. 18, 1893
496,019	—Electric Soldering.....	Apr. 25, 1893
496,020	—System of Electric Distribution.....	Apr. 25, 1893
496,455	—Electric Lighting System.....	May 2, 1893
496,456	—Commutator for Dynamo Electric Machines....	May 2, 1893
496,710	—Friction Coupling for Dynamos or Motors....	May 2, 1893
496,918	—Safety Connection for Induction Coil Systems.	May 9, 1893
497,361	—Commutator Brush Holder for Dynamo Elec- tric Machines.....	May 16, 1893
497,838	—Safety Appliance for Systems of Electric Dis- tribution.....	May 23, 1893
498,327	—Pole Piece for Dynamo Electric Machines....	May 30, 1893
500,629	—Electric Switch.....	July 4, 1893
*500,630	—Method of and Means for Producing Alter- nating Currents.....	July 4, 1893
500,631	—Rheostat.....	July 11, 1893
501,114	—Lightning Arrester.....	July 11, 1893
501,172	—Manufacture of Incandescent Electric Lamps..	July 11, 1893
501,546	—Automatic Chain Welding Machine.....	July 18, 1893
501,547	—Shaping and Spinning Metals by Electricity..	July 18, 1893
502,022	—Electric Measuring Instrument.....	July 25, 1893
502,330	—Fusible Cut-Out.....	Aug. 1, 1893
502,788	—Regulator for Electric Generators.....	Aug. 8, 1893
503,445	—Method of Winding Coils for Dynamo Elec- tric Armatures.....	Aug. 15, 1893
506,383	—Cut-Out.....	Oct. 10, 1893

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
508,646—	System of Electrical Distribution.....	Nov. 14, 1893
508,647—	Electric Lighting System.....	Nov. 14, 1893
508,648—	Lightning Arrester.....	Nov. 14, 1893
508,649—	Protection for the Insulation of Dynamo Electric Machines.....	Nov. 14, 1893
508,650—	Electrical Transformer.....	Nov. 14, 1893
508,651—	Contact Apparatus.....	Nov. 14, 1893
508,652—	Electric Cut-Out.....	Nov. 14, 1893
508,653—	Insulating Composition.....	Nov. 14, 1893
*508,654—	Cooling Transformer.....	Nov. 14, 1893
508,655—	Electrical Transformer.....	Nov. 14, 1893
508,656—	Electric Arc Lamp.....	Nov. 14, 1893
508,657—	Reactive Coil.....	Nov. 14, 1893
508,658—	Dynamo Electric Machine or Motor.....	Nov. 14, 1893
508,659—	Leading-In Wire for Incandescent Lamps....	Nov. 14, 1893
508,660—	Detector for Electric Current Meters.....	Nov. 14, 1893
508,661—	Electric Meter.....	Nov. 14, 1893
508,662—	Indicating Apparatus for Electric Circuit....	Nov. 14, 1893
509,499—	Regulator for Dynamo Electric Machines....	Nov. 28, 1893
511,375—	Method of and Means for Compounding Dy- namo Electric Machines.....	Dec. 26, 1893
511,376—	Electric Measuring Instrument.....	Dec. 26, 1893
512,848—	Chain Making Machine.....	Jan. 16, 1894
513,349—	Means for Neutralizing Self Induction in Alternating Circuits.....	Jan. 23, 1894
*513,602—	Electric Furnace.....	Jan. 30, 1894
516,666—	Electric Railway System.....	Mar. 20, 1894
516,845—	Method of Constructing Commutators for Dynamoes or Motors.....	Mar. 20, 1894
*516,846—	Regulation of Alternating Currents.....	Mar. 20, 1894
*516,847—	Means for Regulating Alternating Currents..	Mar. 20, 1894
516,848—	Armature Winding.....	Mar. 20, 1894
*516,849—	Alternating Current Electric Motor.....	Mar. 20, 1894
516,850—	Electrical Transformer.....	Mar. 20, 1894
518,290—	Armature for Dynamo Electric Machines....	Apr. 17, 1894
*518,291—	Mode of Cooling Electric Motors.....	Apr. 17, 1894
519,076—	System of Electrical Distribution.....	May 1, 1894
520,809—	Means for Preventing Arcing in Electric Power Stations.....	June 5, 1894
520,810—	Electric Reciprocating Motor.....	June 5, 1894
*520,811—	Electric Meter.....	June 5, 1894
*521,684—	Meter for Recording Measurements of Elec- tric Power.....	June 19, 1894
521,685—	Electric Meter.....	June 19, 1894
522,241—	Alternating Current Dynamo Electric Ma- chine.....	July 3, 1894
522,865—	Current Interrupter for High Potential Cir- cuits.....	July 10, 1894
523,019—	Commutator for Dynamo Electric Machines...	July 17, 1894
523,695—	Electro Expansion Device.....	July 31, 1894
523,696—	Dynamo Electric Machine.....	July 31, 1894
525,034—	Electric Arc Lamp.....	Aug. 28, 1894
525,035—	Electric Arc Lamp.....	Aug. 28, 1894
525,369—	Electric Lighting System and Apparatus....	Sept. 4, 1894
526,169—	Electric Apparatus (Motor).....	Sept. 18, 1894
528,188—	Electric Transformer.....	Oct. 30, 1894
529,429—	Electric Incandescent Lamp.....	Nov. 20, 1894

NATIONAL ACADEMY BIOGRAPHICAL MEMOIRS—VOL. XXI

<i>No.</i>	<i>Title</i>	<i>Date</i>
532,838—	Electric Welding Apparatus.....	Jan. 22, 1895
532,839—	Electric Meter.....	Jan. 22, 1895
533,931—	Dynamo Electric Machine.....	Feb. 12, 1895
533,932—	Carbon for Arc Lamps.....	Feb. 12, 1895
*534,730—	Means for Operating Drills.....	Feb. 26, 1895
534,731—	Method of and Means for Preventing Mag- netic Leakage.....	Feb. 26, 1895
537,498—	Incandescent Electric Lamp.....	Apr. 16, 1895
537,499—	Electric Measuring Instrument.....	Apr. 16, 1895
537,500—	Electric Measuring Instrument.....	Apr. 16, 1895
537,501—	Electric Measuring Instrument.....	Apr. 16, 1895
539,453—	Carbon Brush.....	May 21, 1895
539,454—	Carbon Brush.....	May 21, 1895
539,886—	Electric Meter.....	May 28, 1895
540,035—	Brush Holder for Dynamo Electric Machines..	May 28, 1895
542,295—	Transformer for Alternating Current Systems.	July 9, 1895
542,662—	Electric Arc Lamp.....	July 16, 1895
*542,663—	Electric Measuring Instrument.....	July 16, 1895
543,198—	Electric Current Distributor.....	July 23, 1895
543,950—	System of Electric Distribution.....	Aug. 6, 1895
544,396—	Winding of Dynamo Electric Machines or Motors.....	Aug. 13, 1895
545,111—	Means for Synchronizing Electric Motors....	Aug. 27, 1895
545,554—	Alternating Current Generator or Motor....	Sept. 13, 1895
548,406—	Dynamo Electric Machine.....	Oct. 22, 1895
550,464—	Dynamo Electric Machine.....	Nov. 26, 1895
550,733—	Electric Safety Device.....	Dec. 3, 1895
551,799—	Electric Arc Lamp.....	Dec. 24, 1895
554,321—	Electrical Measuring Instrument.....	Feb. 11, 1896
555,130—	Electric Welding Indicator.....	Feb. 25, 1896
555,131—	Electric Riveting.....	Feb. 25, 1896
555,191—	Electric Motor.....	Feb. 25, 1896
555,590—	Monocyclic Generator.....	Mar. 3, 1896
560,379—	Electric Measuring Instrument.....	Mar. 19, 1896
563,895—	Rotary Transformer.....	July 14, 1896
*564,806—	Process of Producing Gas.....	July 28, 1896
571,463—	Controlling Electric Arcs.....	Nov. 17, 1896
574,123—	Electric Arc Lamp.....	Dec. 29, 1896
*575,772—	Roentgen Ray Tube.....	Jan. 26, 1897
*578,430—	Electric Meter.....	Mar. 9, 1897
580,020—	Process of Producing Gas.....	Apr. 6, 1897
580,475—	Electric Riveting Apparatus.....	Apr. 13, 1897
581,873—	Electrical Transformer.....	May 4, 1897
583,955—	Carbon Holder for Arc Lamps.....	June 8, 1897
*583,956—	Producing Stereoscopic Pictures by Roentgen Rays.....	June 8, 1897
*583,957—	Electrostatic Influence Machine.....	June 8, 1897
587,024—	Rectifier.....	July 27, 1897
587,883—	Device for Examining Jewels by Roentgen Rays.....	Aug. 10, 1897
590,653—	Shielding Device for Electric Meters.....	Sept. 28, 1897
590,654—	Electric Measuring Instrument.....	Sept. 28, 1897
591,898—	Damper for Electric Measuring Instruments..	Oct. 19, 1897
*591,899—	Regulating Roentgen Ray Tubes.....	Oct. 19, 1897
595,419—	Method of and Apparatus for Converting Electric Currents.....	Dec. 14, 1897
595,420—	Electric Arc Lamp.....	Dec. 14, 1897

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
596, 190	—Induction Wattmeter.....	Dec. 28, 1897
602, 922	—Electric Arc Lamp.....	Apr. 26, 1898
602, 963	—Distribution of Electric Currents.....	Apr. 26, 1898
610, 928	—Electrostatic Measuring Instrument.....	Sept. 20, 1898
*617, 546	—Controlling Elec. Motors and Trains.....	Jan. 10, 1899
625, 816	—System of Electrical Distribution.....	May 30, 1899
627, 155	—Electrostatic Measuring Instrument.....	June 20, 1899
631, 343	—System of Electric Metering.....	Aug. 22, 1899
634, 965	—Electrical Measuring Instrument.....	Oct. 17, 1899
635, 159	—Electric Meter.....	Oct. 17, 1899
635, 880	—Summation Meter.....	Oct. 31, 1899
635, 881	—Electric Meter.....	Oct. 31, 1899
642, 176	—Internal Combustion Engine.....	Jan. 30, 1900
*645, 675	—High Potential Apparatus.....	Mar. 20, 1900
646, 476	—Carbon Brush.....	Apr. 3, 1900
647, 168	—Safety Appliance for Electric Circuits.....	Apr. 10, 1900
649, 015	—Current Interrupter.....	May 8, 1900
654, 367	—System of Distribution.....	July 24, 1900
655, 032	—Rectifying Alternating Currents.....	July 31, 1900
656, 680	—System of Electrical Distribution.....	Aug. 28, 1900
656, 681	—Circuit Breaker.....	Aug. 28, 1900
659, 716	—Adjusting Reluctance of Mag. Circs.....	Oct. 16, 1900
659, 717	—Contact Device.....	Oct. 16, 1900
*664, 190	—Alternating Current Elec. Motor.....	Dec. 18, 1900
665, 486	—Dynamo Electric Machine.....	Jan. 8, 1901
666, 161	—Elec. Metal Working Apparatus.....	Jan. 15, 1901
666, 162	—Transforming Apparatus for Elec. Metal Working.....	Jan. 15, 1901
667, 106	—Electric Arc Lamp.....	Jan. 29, 1901
667, 107	—Electric Arc Lamp.....	Jan. 29, 1901
669, 291	—Current Interrupter.....	Mar. 5, 1901
669, 670	—Electrical Measuring Instrument.....	Mar. 12, 1901
669, 737	—Gas Engine.....	Mar. 12, 1901
*669, 738	—Vapor Generator.....	Mar. 12, 1901
671, 249	—Rectifier.....	Apr. 2, 1901
*676, 344	—Reg. Device for Alt. Cur. Circuits.....	June 11, 1901
678, 066	—Electric Arc Lamp.....	July 9, 1901
678, 916	—Electric Arc Lamp.....	July 23, 1901
684, 883	—Power Transmitting Device for Engines.....	Oct. 22, 1901
686, 558	—Apparatus for Manufacturing Tubes, Pipes, etc.....	Nov. 12, 1901
687, 588	—Steering Mechanism for Automobiles.....	Nov. 26, 1901
688, 558	—Ignition Tube.....	Dec. 10, 1901
691, 017	—Gas or Oil Internal Combustion Eng.....	Jan. 14, 1902
691, 675	—Manufacture of Electrical Condensers.....	Jan. 21, 1902
*695, 127	—Insulated Conductor.....	Mar. 11, 1902
695, 870	—Electrical Lighting System.....	Mar. 28, 1902
696, 518	—Gas or Oil Engine.....	Apr. 1, 1902
*698, 156	—System of Electric Distribution.....	Apr. 22, 1902
Re. 11, 997	—Insulated Conductor.....	May 27, 1902
701, 965	—Electric Meter.....	June 10, 1902
*702, 038	—Regulation of Speed & Power Engines.....	June 10, 1902
706, 612	—Electric Meter.....	Aug. 12, 1902
712, 106	—Electric Meter.....	Oct. 28, 1902
712, 620	—Electric Meter.....	Nov. 4, 1902
712, 741	—Apparatus for Transferring Elec. Energy.....	Nov. 4, 1902
712, 742	—Alternating Current Meter.....	Nov. 4, 1902

<i>No.</i>	<i>Title</i>	<i>Date</i>
713,023	Electric Meter.....	Nov. 4, 1902
715,901	Controlling Electric Arcs.....	Dec. 16, 1902
716,311	Electrical Conductors.....	Dec. 16, 1902
723,076	Rectifier.....	Mar. 17, 1903
723,189	Rectifying Alternating Currents.....	Mar. 17, 1903
723,502	Internally Fired Engine.....	Mar. 24, 1903
723,503	Internally Fired Engine.....	Mar. 24, 1903
725,798	Multiple Rate Meter.....	Apr. 21, 1903
726,233	Multiple Rate Metering.....	Apr. 21, 1903
726,593	Electric Control Mechanism.....	Apr. 28, 1903
727,713	Means for Accentuating Elec. Contacts.....	May 12, 1903
727,714	Electric Arc Lamp.....	May 12, 1903
729,449	Induction Motor Armature.....	May 26, 1903
729,811	System of Electric Metering.....	June 2, 1903
732,908	Driving Mechanism for Automobiles.....	July 7, 1903
733,093	Means for Regulating the Power of Automobiles.....	July 7, 1903
735,621	Electrostatic Motor.....	Aug. 4, 1903
735,683	Vapor Burner.....	Aug. 4, 1903
739,564	Regulator for Vapor Generators.....	Sept. 22, 1903
*740,203	Fluid Pressure Engine.....	Sept. 29, 1903
*741,388	Steam or Similar Engine.....	Oct. 13, 1903
744,130	Electric Arc Lamp.....	Nov. 17, 1903
745,465	Transparent Refractory Observation Plate.....	Dec. 1, 1903
751,028	Means for Extinguishing Electric Arcs.....	Feb. 2, 1904
755,815	Electric Arc Lamp.....	Mar. 29, 1904
758,157	Means for Preventing Arcing Between Commutator Brushes.....	Apr. 26, 1904
*761,111	Production of Tubes from Refractory Material.....	May 31, 1904
768,636	Driving Mechanism for Self Propelled Vehicles.....	Aug. 30, 1904
*773,827	Roentgen Ray Tube.....	Nov. 1, 1904
774,118	Roentgen Ray Tube.....	Nov. 1, 1904
*775,586	Valve Mechanism.....	Nov. 22, 1904
777,867	Photometric Apparatus.....	Dec. 20, 1904
*778,286	Manipulation of Refractory Material.....	Dec. 27, 1904
779,189	Power Generating Apparatus.....	Jan. 3, 1905
779,190	Static Influence Electric Machine.....	Jan. 3, 1905
781,035	Commutation of Electric Currents.....	Jan. 31, 1905
781,921	Gas or Fuel Engine.....	Feb. 7, 1905
781,922	Hydrocarbon Burner.....	Feb. 7, 1905
783,651	Transformer for Electric Metal Working Apparatus.....	Feb. 28, 1905
789,463	Reactive Coil.....	May 9, 1905
792,087	Thermo-Regulator for Vapor Burners.....	June 13, 1905
792,562	Vapor Burner.....	June 13, 1905
795,422	Means for Preventing Pounding in Internal Combustion Engines.....	July 25, 1905
796,684	Electric Heater.....	Aug. 8, 1905
799,809	Nozzle for Elastic Fluid Turbines.....	Sept. 19, 1905
801,419	Electric Measuring Instrument.....	Oct. 10, 1905
805,248	Electric Lamp.....	Nov. 21, 1905
808,263	Power Transmitting Mechanism.....	Dec. 26, 1905
809,761	Electrostatic Influence Machine.....	Jan. 9, 1906
*822,322	Engine.....	June 5, 1906
822,323	Thermostatic Control.....	June 5, 1906

ELIHU THOMSON—COMPTON

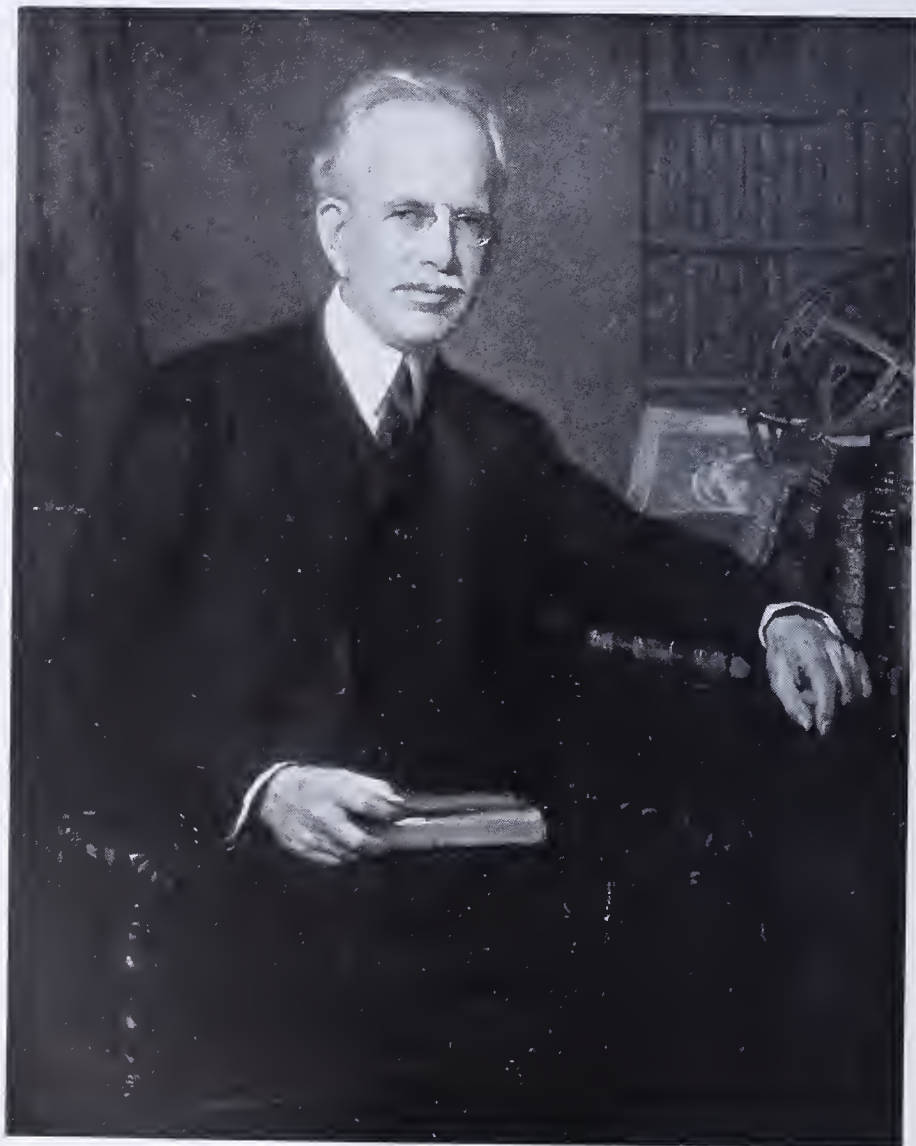
<i>No.</i>	<i>Title</i>	<i>Date</i>
822,324—	Governing Mechanism for Elastic Fluid Tur-	
	bines.....	June 5, 1906
*824,048—	Insulated Coil for Electrical Apparatus and	
	making the same.....	June 19, 1906
832,708—	Diaphragm Actuated Mechanism.....	Oct. 9, 1906
839,436—	Curve Drawing Instrument.....	Dec. 25, 1906
841,356—	Music Sheet Guiding Device.....	Jan. 15, 1907
848,607—	Oil or Gas Engine.....	Mar. 26, 1907
854,777—	Electric Heater 1.....	May 28, 1907
*854,778—	Apparatus for Muffling the Exhaust of Gas	
	Engines.....	May 28, 1907
857,122—	Electric Heater.....	June 18, 1907
859,350—	Unipolar Generator.....	July 9, 1907
877,473—	Torque Regulating Mechanism.....	Jan. 21, 1908
881,502—	Mechanical Movement.....	Mar. 10, 1908
884,539—	Motive Power Engine.....	Apr. 14, 1908
884,540—	Electric Heater.....	Apr. 14, 1908
890,819—	Elastic Fluid Turbine.....	June 16, 1908
892,097—	Vapor Generating Apparatus.....	June 30, 1908
892,196—	Steam Generating Apparatus.....	June 30, 1908
901,498—	Condenser.....	Oct. 20, 1908
902,024—	Electric Heater.....	Oct. 27, 1908
910,743—	Variable Resistance.....	Jan. 26, 1909
917,187—	Electric Measuring Instrument.....	Apr. 6, 1909
920,789—	Elastic Fluid Turbine.....	May 4, 1909
920,790—	Elastic Fluid Turbine.....	May 4, 1909
924,856—	Oil or Gas Engine.....	June 15, 1909
925,055—	Measuring Instrument.....	June 15, 1909
*925,731—	Flexible Coupling.....	June 22, 1909
927,191—	Electric Measuring Instrument.....	July 6, 1909
945,993—	Resistance Unit.....	Jan. 11, 1910
953,241—	Elastic Fluid Turbine.....	Mar. 29, 1910
957,915—	Elastic Fluid Turbine.....	May 17, 1910
960,440—	Compensator.....	June 7, 1910
960,441—	Production of Fine Metal Tungsten.....	June 7, 1910
969,734—	Balancing Means for Turbines.....	Sept. 6, 1910
973,586—	Electrical Welding of Sheet Metal.....	Oct. 25, 1910
980,703—	Inc. Lamp.....	Jan. 3, 1911
984,719—	Electric Welding.....	Feb. 21, 1911
993,910—	Speed Indicator.....	May 30, 1911
996,377—	Electric Measuring Instrument.....	June 27, 1911
996,378—	Changeable Compression Engine.....	June 27, 1911
997,940—	Generating High Temperature Vapor.....	July 11, 1911
1,001,709—	Vapor Electric Apparatus.....	Aug. 29, 1911
1,001,710—	System of Electrical Distribution.....	Aug. 29, 1911
1,003,547—	Transformer Secondary.....	Sept. 19, 1911
1,006,805—	Mercury Vapor Device.....	Oct. 24, 1911
1,008,622—	Electric Heater.....	Nov. 14, 1911
1,010,987—	Make & Break Sparker for Internal Com-	
	bustion Engines.....	Dec. 5, 1911
1,011,526—	Vibrating Rectifier.....	Dec. 12, 1911
1,012,934—	Electric Metal Working Apparatus.....	Dec. 26, 1911
1,015,982—	Regulating & Controlling the Production of	
	Steam.....	Jan. 30, 1912
1,015,983—	Regulation and Control of Steam Production..	Jan. 30, 1912
1,021,219—	Igniting Apparatus for Gas Engines.....	Mar. 26, 1912
1,021,220—	Vaporizer for Internal-Combustion Engines...	Mar. 26, 1912

NATIONAL ACADEMY BIOGRAPHICAL MEMOIRS—VOL. XXI

<i>No.</i>	<i>Title</i>	<i>Date</i>
1,022,517	—Electrical Measuring Instrument.....	Apr. 9, 1912
1,022,712	—Uniting Metals.....	Apr. 9, 1912
1,031,489	—System of Heating.....	July 2, 1912
1,031,490	—Reduction of Ores.....	July 2, 1912
1,039,463	—Electrical Resistance.....	Sept. 24, 1912
1,041,197	—Arc Lamp.....	Oct. 15, 1912
1,045,641	—Vapor Electric Device.....	Nov. 26, 1912
1,045,911	—Electric Metal Working Machine.....	Dec. 3, 1912
1,047,593	—Turbo-electric Ship Propulsion.....	Dec. 17, 1912
1,047,858	—Turbo-electric Propulsion of Vessels.....	Dec. 17, 1912
1,048,915	—Clamp for Electric Metal Working Apparatus.....	Dec. 31, 1912
1,063,303	—Electrical Resistance.....	June 3, 1913
1,063,619	—Steam Power System.....	June 3, 1913
1,072,530	—Electric Heater.....	Sept. 9, 1913
1,075,738	—Repairing Railway Rails.....	Oct. 14, 1913
1,076,467	—Welding.....	Oct. 21, 1913
1,078,225	—Electric Welding of Sheet Metal.....	Nov. 11, 1913
1,080,733	—Valve Mechanism for Engines.....	Dec. 9, 1913
1,080,734	—Condensing Apparatus.....	Dec. 9, 1913
1,083,956	—Electric Seam Welding.....	Jan. 13, 1914
1,084,673	—Spot Welding Machine.....	Jan. 20, 1914
1,085,769	—Spot Welding Thin Sheets.....	Feb. 3, 1914
1,093,159	—Turbo Ship Steadying Device.....	Apr. 14, 1914
1,095,131	—Speed Indicator.....	Apr. 28, 1914
1,095,132	—Power Transmitting Mechanism.....	Apr. 28, 1914
1,096,405	—Internal Combustion Engine and Operating Same.....	May 12, 1914
1,097,895	—Spot Welding.....	May 26, 1914
1,105,047	—Oil Engine.....	July 28, 1914
1,105,716	—System of Distribution.....	Aug. 4, 1914
1,112,238	—Centrifugal Pump.....	Sept. 29, 1914
1,118,382	—Propelling Ships by Polyphase Electric Cur- rent.....	Nov. 24, 1914
1,118,383	—Centrifugal Pump.....	Nov. 24, 1914
1,118,384	—Hydraulic Clutch Mechanism.....	Nov. 24, 1914
1,121,953	—Telephone Metering System.....	Dec. 22, 1914
1,122,665	—Spot Welding Machine.....	Dec. 29, 1914
1,123,624	—Electric Seam Welding.....	Jan. 5, 1915
1,134,776	—Induction Motor.....	Apr. 6, 1915
1,157,344	—Means for Preventing Corona Loss.....	Oct. 19, 1915
1,168,346	—Apparatus for Electric Welding.....	Jan. 18, 1916
1,173,688	—Making a Vitreous Body of Variable Com- position.....	Feb. 29, 1916
1,190,044	—Measurement of Small Pressures.....	July 4, 1916
1,192,706	—X-Ray Tube.....	July 25, 1916
1,220,997	—Combined Spot and Butt Welder.....	Mar. 27, 1917
1,252,201	—Electric Meter.....	Jan. 1, 1918
1,255,667	—High Potential Insulator.....	Feb. 5, 1918
1,256,951	—Cooling Spot Welding Electrode.....	Feb. 19, 1918
1,266,347	—Electric Meter.....	May 14, 1918
1,273,203	—Electric Welding.....	July 23, 1918
1,334,571	—Electric Metal-Working Apparatus.....	Mar. 23, 1920
1,337,106	—Process of Making Nitric Acid.....	Apr. 13, 1920
1,365,567	—Leakage-Prevention Arrangement for Fuel Tanks.....	Jan. 11, 1921
1,375,982	—Condenser.....	Apr. 26, 1921

ELIHU THOMSON—COMPTON

<i>No.</i>	<i>Title</i>	<i>Date</i>
1,375,983—	Electric Switching Device.....	Apr. 26, 1921
1,396,541—	Electric-Battery System and Method of Operating Same.....	Nov. 8, 1921
1,450,464—	Crystal Formation.....	Apr. 3, 1923
1,460,083—	Recording Signal.....	June 26, 1923
1,472,504—	Electric Heater.....	Oct. 30, 1923
1,491,440—	Switching Apparatus.....	Apr. 22, 1924
1,491,441—	High Speed Alternating Current Dynamo Electric Machine.....	Apr. 22, 1924
1,504,002—	Electrostatic Condenser.....	Aug. 5, 1924
1,530,441—	Mirror.....	Mar. 17, 1925
*1,532,002—	Composite Quartz Body.....	Mar. 31, 1925
1,536,948—	Electric Condenser.....	May 5, 1925
1,546,266—	Process of Shaping Fused Silica.....	July 14, 1925
1,548,691—	Line Welding.....	Aug. 4, 1925
1,555,775—	Arc-Lamp-Feeding Mechanism.....	Sept. 29, 1925
1,559,203—	Process of and Apparatus for Purifying Fusions.....	Oct. 27, 1925
1,563,051—	Regulator for Inclosed Electrical Apparatus... Nov. 24, 1925	
1,568,102—	Refrigerating Apparatus.....	Jan. 5, 1926
1,587,445—	Electric Welding or Riveting.....	June 1, 1926
1,588,210—	Arc Interrupter.....	June 8, 1926
1,603,221—	Method and Apparatus for Making Glass.... Oct. 12, 1926	
1,610,182—	Fused Quartz Product and Process of Pro- ducing Same.....	Dec. 7, 1926
1,637,063—	Electric Welding.....	July 26, 1927
1,665,331—	Producing and Reproducing Sound Records... Apr. 10, 1928	
1,680,705—	Method of Making Insulators.....	Aug. 14, 1928
1,683,146—	High Frequency Induction Apparatus.....	Sept. 4, 1928
1,701,346—	Electrolytic Apparatus and Method of Opera- tion.....	Feb. 5, 1929
1,717,281—	Insulator.....	June 11, 1929
1,717,530—	Electric Arc Welding.....	June 18, 1929
1,723,959—	Safety Device for Gas Tanks.....	Aug. 6, 1929
1,746,202—	Electric Welding.....	Feb. 4, 1930
1,746,203—	Method and Apparatus for Electric Arc Welding.....	Feb. 4, 1930
1,746,204—	Electric Welding.....	Feb. 4, 1930
1,746,205—	Electric Arc Welding.....	Feb. 4, 1930
1,759,767—	Liquid Level Gauge for Boilers.....	May 20, 1930
Re.17,826—	Cooling Container.....	Oct. 14, 1930
1,843,792—	Composite Silica Article.....	Feb. 2, 1932



George E. Hale

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BIOGRAPHICAL MEMOIR

OF

GEORGE ELLERY HALE

1868–1938

BY

WALTER S. ADAMS

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George Ellery Hale, distinguished scientist and for many years one of the leading members of the National Academy of Sciences, was born in Chicago on June 29, 1868. His family can be traced backward to Thomas Hale, a farmer of some property who lived at Watton-on-Stone, Hertfordshire, England. He died in 1630 and his son, also named Thomas, emigrated to America about 1640 and settled at Newbury, Massachusetts. Later he moved to Haverhill and must have been active in town affairs, since in 1646 he was chairman of the Board of Selectmen. Successive generations of sons lived in Newbury and vicinity and were numbered among the merchants, carpenters, and weavers of that slowly growing New England community.

The great-grandfather of George Hale, Benjamin by name, moved to Boston and kept the Eastern Stage House, an inn frequented by sea-faring folk and mentioned in King's Handbook of Boston. His son, Benjamin Ellery, was born in 1809 and became a Congregational minister, occupying pastorates in several Massachusetts towns. He became greatly interested in the cause of temperance, delivering many lectures on the evils of alcohol; and when he later moved to Hartford, Connecticut, and gave up the ministry owing to failing eyesight, he organized an insurance company which limited its risks to people of temperate habits. In 1854 he went to Beloit, Wisconsin, and took a position with the Rock River Paper Company. When this concern moved to New York he went with it and lived for some time in Brooklyn. He died in 1882.

Benjamin Ellery Hale was married twice and of the six children of his first marriage William Ellery, father of George, was born on April 8, 1836. He received his early education in the public schools of Hartford, Connecticut. A few years after his parents moved to Beloit he went to Chicago as agent for the Rock River Paper Company and became interested in that rapidly developing city. He erected a building at the corner

of State and Washington Streets which was destroyed in the great fire of 1871. During its reconstruction the possibilities of hydraulic elevators attracted him greatly, and in partnership with his brother George W. Hale he established the firm of William E. Hale and Company to manufacture and install passenger elevators under patents purchased from Cyrus W. Baldwin. The firm prospered, and when some 20 years later the two brothers sold their interests to the Crane Company, each had acquired a considerable fortune.

In 1862, at about the time William E. Hale came to Chicago, he married Mary Browne, daughter of Dr. Gardiner S. Browne of Hartford, Connecticut. Five children were born of the marriage and of these three survived: George Ellery, born June 29, 1868; Martha Davis, born July 28, 1870; and William Browne, born December 7, 1875. George Hale was born in a house on La Salle Street, but a year before the great fire his father moved to the southern part of the city. Here in 1885 he built a stone house of considerable size at the northeast corner of Drexel Boulevard and Forty-Eighth Street.

Although not a college graduate, William E. Hale had a deep appreciation of the value of education to the individual and to society and was generous in its support. He became a trustee of Beloit College and contributed the endowment for a hall of science at that institution. Most interesting of his characteristics, however, and of those of his wife as well, were their ability to recognize the rare intellectual gifts of their son George and the constant encouragement which they afforded him as his mind began to unfold and his interests to develop. In later years Hale referred again and again to the decisive part which his father and mother took in this formative period of his youth.

It is an extremely interesting picture which we can form of Hale in the early years of his life. Born with a quick and active mind and an insatiable curiosity about every fact and process in the physical world about him, he had the constant sympathy and encouragement of a father who helped to develop his natural interest in tools and methods of work and aided him to establish at a very early age an intelligent knowledge of his relations to the community; and of a mother who formed his tastes in reading and opened for him all the delights of the poetry and litera-

ture of the past. His fondness for poetry began with the *Iliad* and *Odyssey* and developed more and more strongly throughout the years; and to his wide reading during his early life must be ascribed the development of his creative imagination, a characteristic which aided and enriched so greatly his later scientific work.

So we see the young boy transforming his bedroom and later a small hall-room into a little shop, surrounded by his tools, his scroll saw and a simple wood lathe. One Christmas day his father gave him a lathe for turning metal, and these early interests in machinery and the construction of instruments remained with him throughout his life. A small microscope was another of his most highly prized possessions, and with his younger brother and sister he made many a collection of rotifers and infusoria from the stagnant ditches of Kenwood for examination. Already he was learning the fascination of the marvelous worlds hidden from the unaided eye. He delighted in such books as "The Young Mechanics" and "The Boy Engineers", both published in London, and he and his brother were soon led through the father's encouragement to build with their own hands a shop of their own. In a room ten by fifteen feet in size they installed their tools and work benches, built a small horizontal steam engine and finally with the aid of a second-hand boiler had the great joy of operating a power-driven lathe. The building also served the purposes of a tiny physical laboratory.

The use of his first microscope in the study of infusoria had interested the young boy in optics. This interest was greatly deepened by the acquisition of a fine Beck instrument which his father had given him after solemnly submitting him to a successful examination upon the results of his work. He soon obtained a small camera with which he made photographs of microscopic objects, and other possibilities at once began to present themselves. His first telescope he built in his own shop, but the simple lens used naturally failed to give good images. At about this time he became acquainted with S. W. Burnham, who served as stenographer in the Chicago law-courts by day and observed double stars by night. Through Burnham's aid Hale's father purchased a second-hand Clark refractor which was mounted on the roof of the house at Kenwood, and the 14-year-old boy

was amazed and delighted with the views of the moon and planets afforded by the excellent lens. He promptly began making photographs, observed a partial eclipse of the sun, and commenced drawings of sun-spots. His feet had entered upon the long road of scientific research.

It is clear from this brief summary of Hale's earliest years that his interests had already become defined. They lay in physics and physical applications, in instruments and experimental methods, and not in the more formal types of observation of classical astronomy. He became an enthusiastic disciple of the youthful science of astrophysics, and his prophets were Huggins, Lockyer and C. A. Young. He soon built a simple spectroscope in his shop, and his interest and excitement in observing the solar spectrum and measuring the principal lines remained in later life as one of the most vivid memories of his boyhood. Up to the time of his death Hale could never look at a high-dispersion solar spectrum without an intense feeling of pleasure and anticipation. During these early years he read avidly all he could find regarding spectra, and the greatly worn copy of Lockyer's "Studies in Spectrum Analysis" still remaining in Hale's library bears witness to the extent to which it was used by the enthusiastic boy. He soon learned of the work of Rutherford and Rowland, and in the larger laboratory in the upper story of the new house built by his father in 1885, Rutherford's long photographic map of the solar spectrum hung on the wall near the grating spectroscope.

Although this picture of Hale's boyhood emphasizes his interest in science and scientific instruments, he was altogether a normal boy in his pleasures in life. He fished and swam and skated, played tennis, rode a bicycle, and built both a canoe and an ice-boat. He even gave magic performances, building most of the "properties" used in his exhibitions. He read Jules Verne's stories with great enthusiasm and took especial delight in certain tales of adventure laid in California, the mountains of which seemed to attract him even at that time. His summers he nearly always spent at his grandmother's house in Madison, Connecticut, a pleasant old New England village where time had apparently stood still and ox-carts still moved about the streets. The contrast between the busy life of Chicago and the

placid life of the inhabitants of the little country town on Long Island Sound interested George Hale greatly, and he often spoke in later life of his satisfaction in having known a little of the Puritan civilization before it had so nearly disappeared.

Not far from his grandmother's home in Madison was the house of the mother of Evelina Conklin and there he met his future wife whose affection and understanding and courage made possible the long and illustrious career which lay before him.

Hale's individualism and his interest in tasks of his own selection he carried with him into his school life. After passing through the Oakland Public School in Chicago he entered Allen Academy where the principal, Ira W. Allen, recognizing the boy's talents, gave him unofficial charge of the very limited physical apparatus and allowed him to serve as assistant in demonstrations before the classes. Courses in elementary chemistry, physics and astronomy supplemented his home reading, and an additional course in shop-work at the Chicago Manual Training School gave him valuable experience in a field in which he was already competent. Meanwhile his reading and activities in directions other than physics and astronomy had widened his interests greatly. "The Origin of Species" affected him profoundly and gave him a lasting desire to apply evolutionary principles to other fields of science. His father had been closely associated in business with the well-known architect Daniel H. Burnham, and through him Hale acquired a knowledge of the principles of architecture and city planning which he put to excellent use in later years. It was on Burnham's advice that in 1886 at the age of seventeen Hale selected the Massachusetts Institute of Technology for his later studies.

#### 1886-1892

The four years at the Massachusetts Institute were exceedingly busy ones. The course of study in physics together with the chemistry and mathematics, which he had selected, occupied his time very fully, but he still found it possible to continue his astronomy at the Harvard College Observatory where Edward C. Pickering allowed him to serve as volunteer assistant. He did much reading of original articles on astronomy and spectroscopy in the Boston Public Library. His interest in art and

music developed greatly during this period, and the beginnings of his life-long friendship with his classmate Harry M. Goodwin gave him a companion of similar views and tastes.

Before Hale left Chicago to enter the Institute he had designed a spectroscopic laboratory which was soon built on a lot adjoining the house in Kenwood. The equipment included a heliostat and a spectrograph of ten feet focal length with a Rowland concave grating. This instrument he used during his summer vacations in the study of arc, spark and solar spectra. However, having seen a solar prominence for the first time with C. A. Young's telescope, his active mind began to search for a method of photographing prominences in full daylight and thus obtaining a full and permanent record of these important solar phenomena. This was the beginning of the spectroheliograph. The first instrument, which was used with a horizontal telescope at the Harvard Observatory in the winter of 1889-90, did not give satisfactory photographs because of imperfections in the instrument, lack of guiding mechanism, and distortion of the solar image, but it was clear that the principle and method were fully adequate. When he returned to Chicago his father offered to provide a telescope of sufficient size to carry the spectroheliograph, and a 12-inch refractor with lens by Brashear and mounting by Warner and Swasey was ordered. The building and dome together with a room for a library were erected against the south front of the existing spectroscopic laboratory, and the completed equipment came to form the Kenwood Observatory.

During the period of construction of the telescope Hale worked in his laboratory. A visit to the Lick Observatory, which he had made immediately after his marriage in June 1890, had impressed him enormously with the possibilities of a powerful spectrograph on a large telescope in the hands of such skilled observers as Keeler and Campbell. As a result of Keeler's investigations on the spectra of planetary nebulae, Hale made a study of the bands of magnesium and published his conclusions in a paper in the *Sidereal Messenger*, January 1891. He had previously published several short articles dealing mainly with stellar and solar photography in the *Beacon, Technology*

*Quarterly* and *Astronomische Nachrichten*. His earliest known publication was in March 1889.

As soon as the new instruments were ready for use in the spring of 1891, Hale began active work with his spectroheliograph. He discovered in the spectra of prominences the remarkable brightness of the H and K lines, which he had found to be due to calcium, and thus had at hand an ideal means of photographing solar phenomena with existing plates. It was not until years later that red-sensitive emulsions made possible the use of the  $\alpha$  line of hydrogen as well. He obtained some satisfactory photographs of prominences at this time and as a result of experience designed an improved form of spectroheliograph with two moving slits driven by a clepsydra which was ordered from Brashear. While the instrument was under construction he left for a trip to Europe with Mrs. Hale.

The purpose of this trip was twofold. First, he wished to acquaint himself with many astronomers with whose published work he was familiar and to study their methods and equipment. Secondly, he wished to secure their views and, if these were favorable, their support for the new astrophysical publication which he had in mind—a journal in which physicists and astronomers would find a common meeting ground. In both objects he was remarkably successful. Astronomers and physicists in England and Continental Europe were only too glad to discuss their problems with the brilliant young man whose inventive skill was already well known to them. They also joined heartily in the plans for the new publication, and on his return from Europe with the endorsements of a score of eminent scientific men he started the journal at first known as *Astronomy and Astrophysics*. For three years it represented a combination with the *Sidereal Messenger*, but beginning with 1895 this plan was discontinued and the *Astrophysical Journal* began its long and noteworthy career. Hale and Keeler were its first editors, supported by a strong board of collaborating scientists.

On his return to Chicago, Hale began active work with the new spectroheliograph which proved to be a most successful instrument. Photographs of solar spectra had shown that the H and K lines are bright, not only in the chromosphere and prominences, but also in the vicinity of sun-spots and over

irregular regions scattered over the sun's surface. With the spectroheliograph he now succeeded in photographing over the entire sun these calcium clouds to which he gave the name of "flocculi." The systematic study of the flocculi has proved of great importance ever since their discovery, and the probable relationship of rapidly developing and changing flocculi and disturbed areas near sun-spots to magnetic storms on the earth was found by Hale in July 1892. Most of his results are given in publications in *Astronomy and Astrophysics*. The extent of the work at the Kenwood Observatory is indicated by the fact that more than 3000 spectroheliograms were taken by Hale and his assistants, more especially by Ferdinand Ellerman who came to Kenwood in 1892. In addition to his observing Hale found time to organize the astronomical exhibit at the Columbian Exposition in Chicago in 1893, and to act as secretary of the section of astronomy and astrophysics of the international congress held at that time.

#### 1892-1903

Wider plans had begun to develop in Hale's mind by this time. The University of Chicago had recently been founded by John D. Rockefeller, and although it had not yet been formally opened, its wise and far-sighted President, William R. Harper, had already appointed Hale to its faculty. During a meeting in Rochester, New York, of the American Association for the Advancement of Science Hale had met Alvan Clark, who told him of two 40-inch disks of optical glass, originally ordered for the University of Southern California but now, owing to financial difficulties, once more available for purchase at cost. The possibility of securing these for the new University of Chicago at once attracted Hale, and President Harper was in full agreement. Several Chicago business men were appealed to on the subject, and finally Charles T. Yerkes agreed to provide the funds for the purchase of the disks and the completion of the optical work. Soon afterward, influenced by Hale's enthusiasm and persuasiveness, he also promised to provide for the mounting of the telescope. No provision, however, was made for the building and other equipment, and it was only after a long delay, when Hale was again in Europe, that Yerkes finally consented to construct the observatory building. Much

of this was designed by Hale in correspondence with Henry Ives Cobb, the University architect.

The second European visit was partly for pleasure but mainly for research and further study. In England he renewed many of the associations he had made on his earlier visit and added greatly to the number of his friends. Especially in the case of Professor Newall of Cambridge, a close friendship was established which lasted throughout Hale's life. At Berlin he attended lectures at the University by Planck, Rubens and others, and with his friend Goodwin attempted some investigations with photoelectric cells. His chief interest, however, lay in the astrophysical work in progress at Potsdam, and in the course of many visits he became intimately acquainted with the investigations of Vogel, Scheiner, and the other members of the able group of that period. Nevertheless, neither the climate nor the German food agreed particularly well with Hale, and at the end of a single semester he and Mrs. Hale started for Italy by way of Vienna.

The change from the cold and darkness of northern Europe to the warmth and sunshine of Italy impressed Hale enormously. As he often said, he was always a true son of the south. He spent a few days in Venice, visited Florence where the Astrophysical Observatory was under construction and where he discussed solar problems with Tacchini and the elder Abetti, and then journeyed down through Rome to Southern Italy and Sicily. At Catania he met a fellow solar physicist in Riccò, and with him planned an ascent of Etna. A year earlier Hale had tried with the spectroheliograph to photograph the solar corona without an eclipse, both at Kenwood and on Pike's Peak, but without success. With the blue skies above the summit of Etna he hoped for better observing conditions. The spectroheliograph was attached to the equatorial telescope of the Bellini Observatory (elevation 9600 feet), and the second slit of the instrument was set on the broad K line for the purpose of reducing the intensity of the sky spectrum near the sun and recording the coronal streamers by means of their continuous spectrum. The results, however, were negative, although the two scientists spent a week in the attempt. In the last years of his life Hale had the great satisfaction of learning of a

successful solution of this problem through a somewhat different method by Lyot in France.

On his return to Chicago, Hale became immersed in the difficult financial problems of the Yerkes Observatory. No endowment had been given for equipment or maintenance, and the heavy demands upon the University of Chicago prevented it from devoting more than very limited funds to the operation of the Observatory. Hale was obliged to seek gifts in small amounts from every possible source and to conserve his small resources in every way. With courage and hope, however, he built up a strong staff, bringing Barnard from the Lick Observatory and Wadsworth from Michelson's laboratory at the University. Burnham, who had returned to Chicago from Mount Hamilton, gladly undertook observations of double stars two nights each week, and Ellerman came with Hale from Kenwood. Ritchey, a teacher in the Chicago Manual Training School, was engaged privately for optical work at a somewhat later date. Hale's greatest disappointment was his inability to secure Keeler as stellar spectroscopist, the Directorship of the Lick Observatory having been offered to Keeler at just this time. Edwin B. Frost was engaged for this position not long afterward. All the Kenwood equipment, including the 12-inch telescope and dome and the machine tools, was given to the Yerkes Observatory, the small instrument shop was organized, and little by little additions were made to the limited apparatus available at the beginning.

Hale's scientific work was of necessity considerably interrupted during this period by the many difficult responsibilities which he was carrying, but with the completion of the large Rumford spectroheliograph which he had designed he had a powerful instrument for continuing his solar investigations. With its aid he soon discovered dark hydrogen flocculi and the comparatively rare dark calcium flocculi, and by using different portions of the broad K line was able to study the distribution of calcium vapor at different levels in the sun's atmosphere. These results, combined with measurements of the radial motions of the gases in the flocculi, added much to our knowledge of circulatory processes in the sun. A description of the spectroheliograph, together with a discussion of the observa-

tions and many illustrative plates, is found in *Publications of the Yerkes Observatory*, Volume III, Part I. In the same publication Hale also discusses the relative value of various types of investigations in solar physics and formulates some of the principles which governed much of his work in later years.

The spectroscopic study of sun-spots, prominences and the solar chromosphere had always interested Hale greatly. The short-focus grating spectrograph attached to the 40-inch refractor formed an admirable instrument for observations which did not require very high dispersion, and with it he observed widened lines in sun-spots, discovered the bright lines of the green carbon fluting at the edge of the sun, and obtained some excellent photographs of the ultraviolet spectrum of prominences and the chromosphere. It was clear, however, that for much of this work larger and more powerful spectrographs than could be attached to the 40-inch refractor were needed. Accordingly his mind turned to the possibilities of fixed coelostat telescopes which would at the same time provide large images of the sun and make possible the use of spectrographs of any desired length. The results of his thought on this problem were embodied in the horizontal telescope built at the Yerkes Observatory in 1902. This was partially destroyed by fire, but through the gift of Miss Helen Snow of Chicago a larger and more complete instrument took its place. In 1904 the Snow telescope was brought to Mount Wilson and through purchase became a permanent part of the equipment of the new observatory established soon afterward. The development of this instrument into the tower telescope forms an exceedingly interesting illustration of Hale's remarkable appreciation of observational needs and of his resourcefulness in meeting them.

Although primarily interested in solar physics, Hale always realized that the sun is a typical star and was quick to apply to the field of stellar spectroscopy any results found from a study of the sun. Similarly, throughout his life, he was a strong advocate of the value of a spectroscopic laboratory in which to study spectra under controlled conditions and imitate, at least in part, the phenomena found in sun and stars. Several of his most brilliant discoveries of later years were greatly aided by the fact that laboratory resources were available for purposes

of test and comparison. It is of interest to note that two of his principal publications while at the Yerkes Observatory illustrate these conceptions most clearly. In his monograph on "The Spectra of Stars of Secchi's Fourth Type" (with Ellerman and Parkhurst) he dealt with a class of stars of low temperature showing certain marked similarities of spectrum to that of sun-spots; and in the paper "The Spectrum of the High-Potential Discharge between Metallic Electrodes in Liquids and in Gases at High Pressures" (with Kent) he made a laboratory study of spectral phenomena which seemed at that time to bear a striking resemblance to certain features of the spectra of stars and especially novae.

In concluding this brief account of Hale's years at the Yerkes Observatory I can fortunately add a brief statement in his own words of the principal ambitions governing his scientific investigations of this period.

- (1) To continue the development of the spectroheliograph and learn more of the nature of the flocculi and the possible terrestrial effects of solar eruptions.
- (2) To study, under the highest possible dispersion, the spectra of various solar phenomena, especially sun-spots and the chromosphere.
- (3) To continue my investigation of stellar evolution and to photograph the spectra of the brighter stars on a scale as great as that of Rowland's photographs of the solar spectrum.

The first two of these desires Hale fulfilled abundantly within the next few years, but the third he was obliged, because of insufficient strength, to leave to others.

#### *1903-1922*

The announcement in 1902 of the gift by Andrew Carnegie of ten million dollars to establish an institution devoted to pure research came to Hale quite unexpectedly, but he immediately foresaw the possibilities which it presented. After years of difficult and often disappointing search for funds to equip and maintain the Yerkes Observatory, the possible chance that funds might be available for establishing a solar observatory in the

best possible climatic location free from any considerations due to affiliation with an educational institution stirred his mind to the very depths. Here was an opportunity not only to undertake solar research under the most favorable conditions but to plan the equipment to fit the problems in view. His first contact with the Carnegie Institution was as secretary of the Advisory Committee on Astronomy of which Pickering was chairman. Its duties were primarily to survey the field of astronomy and make recommendations for a number of small grants. This function, however, did not entirely satisfy the conceptions of one of the leading members of the Executive Committee, Charles D. Walcott, nor was it altogether in keeping with Mr. Carnegie's own views. Accordingly in a letter to the Committee, Walcott expressed the hope that not only would minor grants and problems be considered but that major projects involving large expenditure be suggested as well. The sole criterion to be considered was the merit of the proposed work. As a result the report of the Advisory Committee contained, in addition to recommendations of appropriations in aid of individual investigations at existing institutions, a proposal for the establishment of a southern observatory with a large reflector for stellar observations, and of a solar observatory at a high altitude in as favorable a climate as possible.

At the meeting of the Executive Committee favorable action was taken upon various individual grants and a special committee, consisting of Lewis Boss, W. W. Campbell and Hale, was appointed to advise on the proposed southern and solar observatories. An appropriation was also made to provide for the study of possible sites in the northern and southern hemispheres by W. J. Hussey of the Lick Observatory. The full report of this committee, together with a detailed statement by Hussey of the results of his investigations of sites, appeared in the Year Book of the Carnegie Institution for 1903. The project of the two observatories was approved in principle by the Executive Committee, and although the income of the Institution was insufficient to provide for the solar observatory as planned by Hale, which was to include a large reflecting telescope for the study of stellar evolution, it was believed that a special gift by Mr. Carnegie would be made for this purpose. Mount Wilson

had provisionally been selected as the site and the directorship had been offered informally to Hale.

In the autumn of 1903 Mrs. Hale accompanied by the two children came to Pasadena for the winter. The elder child Margaret had suffered severely from repeated attacks of bronchitis and asthma in the severe climate of Wisconsin and the journey was made in the hope that the milder weather of the Pacific Coast would prove beneficial. Hale, himself recovering from an illness, remained in Chicago, awaiting with deep interest the final action of the trustees of the Carnegie Institution.

The report of the meeting of the trustees came as a severe disappointment. Various grants were made up to the limit of the available income, but no mention was made of the solar observatory or of any additional gift. Hale considered the situation deeply and with characteristic courage and hope decided to join his family in California and to investigate thoroughly the possibilities for solar research on Mount Wilson. He had visited the mountain in the previous June with Hussey and Campbell and had been immensely pleased with the conditions as they then appeared to him. Arriving in Pasadena on December 20, 1903, he soon made his second ascent, taking with him a small portable telescope. Observations with this instrument convinced him of the value of a larger telescope, and partly through personal contributions and partly through gifts from friends, he secured sufficient funds to bring from the Yerkes Observatory a coelostat telescope with a 6-inch lens having a focal length of 60 feet. He also succeeded in interesting John D. Hooker of Los Angeles in the plan for bringing Barnard with the Bruce 10-inch photographic telescope from the Yerkes Observatory to complete a photographic atlas of the Milky Way, and a gift was made by Mr. Hooker for this purpose.

There now began a novel and extremely interesting and enjoyable period in Hale's life. Depression vanished quickly in the brilliant skies of the mountain top, and the isolation and beauty of the natural surroundings made a strong appeal to many of his most deeply seated instincts. Two narrow trails were the only means of access to the mountain, and transportation of supplies and instruments was wholly by pack trains made

up of burros and mules of ancient lineage. The only building on the mountain top was an old log cabin, badly out of repair, which the owners of the land on the summit kindly allowed Hale to occupy as a dwelling house. Repairs on this building, the installation of a large fireplace, and the construction of the piers for the coelostat telescope occupied Hale and his able assistant George D. Jones during two of the winter months. This was the beginning of the long connection of Jones with the Observatory: in later years as superintendent of construction he erected nearly every important instrument and building of its extensive equipment. In all this work Hale took an active and enthusiastic part, not hesitating to walk down the 9-mile trail in order to ride his bicycle into Pasadena after some needed supplies which he would then carry on his back up the mountain.

In March of 1904 Hale brought Ellerman from the Yerkes Observatory and together they erected the coelostat and 60-foot telescope. At first the beam of light from the coelostat was enclosed in a long tube of building paper protected by a canvas shield, but it was soon evident that the heating of the air in the tube made the image blurred and indistinct. The top of the tube was then removed and the canvas was stretched as a flat shield to the eastward, preventing direct sunshine from striking the tube. A very marked improvement resulted and on April 11 some excellent direct photographs of the sun were obtained. Hale soon afterward started down the mountain to prepare for his journey to Washington to attend the meeting of the National Academy of Sciences, and the completed negatives were brought to him by Ellerman on the afternoon of the same day. The next morning Hale started for Washington, taking the photographs with him.

In addition to his general desire to be present at the Academy meeting, Hale had two specific objects in mind. The first was to present to the Council of the Academy a plan he had conceived for the organization of the International Union for Cooperation in Solar Research: the second was to interest the trustees of the Carnegie Institution in a plan for bringing the Snow telescope from the Yerkes Observatory to Mount Wilson on an expeditionary basis. Both projects met with a favorable reception. The plan for the International Union was approved

by Alexander Agassiz, President of the Academy, and by Simon Newcomb, and was favorably acted upon by the Council. A grant of \$10,000 to bring the Snow telescope to Mount Wilson was made by the trustees of the Carnegie Institution, and several of the leading trustees, including J. S. Billings, Walcott and Weir Mitchell, expressed great interest in the project and held out the hope that additional funds might be granted at the annual meeting of the Institution in December. On his return from Washington to Chicago, Ha'e considered the entire situation most carefully and decided to add personally to the Institution appropriation such funds as seemed necessary to carry out his plans. These, however, were still limited to the conception of an expedition from the Yerkes Observatory and did not provide for the construction of the 60-inch telescope.

Early in May, Hale left Chicago for California taking with him two additional members of the Yerkes group, W. S. Adams and G. W. Ritchey. These with Ellerman, who had remained at Mount Wilson, and F. G. Pease who came about a year later, formed for several years the Mount Wilson staff. A small machine shop was established in Pasadena with Ritchey in immediate charge, and on Mount Wilson construction was begun upon the piers and building to support and house the Snow telescope. It was a period of intense physical and mental activity for Hale and one which he enjoyed beyond measure. The quiet and isolation of the mountain top appealed to him enormously and in the thousand details of planning and construction under pioneering conditions he found an interesting opportunity to apply his resourcefulness and inventive skill. He designed a special small horse-drawn truck, steered from either end, for transporting the heavier parts of the Snow telescope up the narrow foot-trail two feet in width; he planned the living-quarters for the staff, the first "Monastery," on a site selected after a strenuous afternoon devoted to cutting trails through the thick brush; he studied the water supply and means for developing it; but most of all he investigated in great detail methods for improving the definition of the sun's image, especially some hours after sunrise when radiation from the heated ground became most injurious. Tests of the "seeing" at different elevations above the ground, the effect of shielding the ground

in the neighborhood of the beam incident upon the telescope, stirring with fans of the air traversed by the beam from the coelostat mirrors, and artificial heating of the backs of these mirrors to compensate for the distortion produced on their front surfaces by the sun's heat—all of these matters were studied with great care by Hale, and the conclusions were incorporated into the design of the building for the Snow telescope, and in later years into the design of the 60-foot and 150-foot tower telescopes. Although he felt a deep sense of responsibility at this time for the successful outcome of this development work as affecting his future recommendations to the Carnegie Institution, he always retained his hopefulness and courage and faced all his problems with a joyous light-heartedness which was a constant delight to his associates. The small group on Mount Wilson had been increased by the addition of Barnard who had brought the Bruce photographic telescope from the Yerkes Observatory to photograph the southern Milky Way, and the gatherings around the fireplace at the Monastery on the stormy evenings of the winter of 1904-5 with Hale present form one of the choicest memories of these early years of the history of Mount Wilson.

In the autumn of 1904 Hale went to St. Louis where as chairman of a committee of the National Academy he was making preparations for an international meeting at the Exposition called to organize the International Union for Cooperation in Solar Research. After this meeting he visited New York and Washington where he planned to report to Billings and Walcott, trustees of the Carnegie Institution, upon the results of his study of conditions on Mount Wilson. For this purpose he had much material available in the form of direct photographs of the sun taken with the 60-foot focus telescope, and records of day and night seeing over a period of nearly a year. His interview with Billings was one which Hale looked back upon with considerable amusement in later years, but at the time it seemed far from reassuring. Although, as Billings afterward acknowledged, he had a deep and favorable interest in the Mount Wilson project, he took a somewhat grim and pessimistic attitude toward Hale's detailed statement of his plans which would have disconcerted anyone less enthusiastic

and fully convinced of the value of the case he was presenting. Together they visited Mrs. Draper, and Hale then went to Washington to interview Walcott. From him Hale received strong encouragement to revive the original project of a large solar observatory including the 60-inch reflector. The following day, at a meeting of the Executive Committee of the Trustees, Hale on the invitation of Walcott presented two plans, the first providing simply for a continuation of the expedition with the Snow telescope from the Yerkes Observatory, and the second for the establishment of an independent solar observatory and the construction of the 60-inch reflector. It was about two weeks later that Hale, on his way up Mount Wilson, received word over the decrepit single-wire telephone line running to Pasadena that the Executive Committee had acted favorably upon the larger project, had appropriated \$150,000 for each of two years, and had authorized Hale to proceed with the immediate execution of the plan.

The establishment of a new observatory as a department of an institution devoted wholly to research came to Hale as the culmination of hopes and wishes held throughout many years. With his usual modesty he doubtless underestimated his capacity as a teacher, but there can be no question that his interest in research was the dominating influence of his life. Even the development and organization of strong departments of graduate study at the universities with which he was associated, made no such appeal to him as did the opportunity of carrying on his individual investigations under the conditions afforded him by the Carnegie Institution. Here he found for the first time the means of planning and building his equipment with a view to the problems in mind just as the physicist in the laboratory sets up his instruments for definite and specific experiments.

The problem of the 60-inch reflector was one of the first to be undertaken. The glass disk, for the purchase of which funds had long before been provided by Hale's father, was brought from the Yerkes Observatory, a new optical and instrument shop was built in Pasadena, and the figuring of the mirror and the design of the mounting and dome were commenced by Ritchey. The transportation of this material up Mount Wilson presented serious difficulty, and after much consideration of

various possibilities, including that of a telpher cable line, Hale finally decided upon the widening of the existing foot trail into a road. This work was carried out in 1907 and 1908, the long and somewhat dangerous task of transporting the mounting and mirror on a primitive motor truck, supplemented by mule power, was accomplished successfully, and the first tests of the completed telescope were made in December, 1908. They proved extremely satisfactory, and the way to one of Hale's cherished ambitions, the physical study of stellar spectra and of the evolution of stars, now seemed to lie open before him.

In the meantime the Snow telescope was in regular operation with a large spectroheliograph of special design. To aid in the interpretation of the results found with this instrument, a special study was made of the behavior of the H and K lines over the flocculi and other regions of the sun's surface with an 18-foot spectrograph. Two determinations of the rotation period of the sun were also completed by Hale and his colleagues: the first from the motions of the flocculi (to aid in the measurement of which Hale designed a special projection instrument called the heliomicrometer); and the second, a spectrographic determination based upon the Doppler displacements of lines at opposite limbs of the sun. A more important and, in its future application, extremely fruitful investigation was that on the spectrum of sun-spots which was begun in 1906. Visual observations of widened lines had been made previously at several observatories, but photographic methods were only just beginning to be applied. With the excellent definition often found at Mount Wilson a great amount of new and interesting material became at once available on the photographs, and their study became a major investigation of the next few years.

The Snow telescope with its specially designed house of louvre walls was yielding excellent results, but still Hale was not satisfied with the duration of the good seeing conditions. For some two to three hours after sunrise the sun's image would remain sharp and clear, but then air waves rising from the heated ground and to some extent disturbances within the telescope house would produce blurring and lack of definition. To reduce and at least partially eliminate these effects, and because he had larger spectrographs in mind, Hale conceived the tower

telescope, the first of which was built in 1908. In this type of instrument the coelostat on top of the tower receives the sunlight well above the air waves near the ground, and the image-forming lens sends a vertical beam (much less sensitive to disturbances than a horizontal beam) downward to the ground level where the image is produced. A long vertical spectrograph is placed in a deep underground pit and receives the benefit of reasonably constant temperature. Another feature of this telescope was the use of very thick mirrors to resist deformation by the sun's heat. With the marked improvement in the solar definition afforded by this instrument, especially toward the middle of the day, with a 30-foot spectrograph in the vertical pit, and with ease and convenience of operation, the 60-foot tower telescope soon became one of the major solar instruments on Mount Wilson and a model for instruments of similar type in other parts of the world.

Hale had always believed strongly in the great value of affording facilities to astronomers and physicists to undertake special investigations at any location where instrumental or climatic conditions were especially favorable for their work. With this in mind he invited C. G. Abbot in 1906 to undertake measures of the solar radiation at Mount Wilson, E. F. Nichols to study very long-wave radiation in the sun's spectrum, W. H. Julius to investigate anomalous dispersion with the spectroheliograph, and H. G. Gale to undertake spectroscopic work in the small physical laboratory then available on the mountain top. He also gained Kapteyn's assent to becoming a research associate of the Observatory and affording his invaluable advice and judgment regarding problems of stellar research. The result of Hale's policy was to keep the comparatively isolated staff on Mount Wilson in close touch with many of the men and problems which counted for most at this period in the history of astrophysics.

During these years of intense scientific activity, the busiest and probably the most enjoyable of Hale's life, two investigations among the many in which he was engaged stand out with especial prominence. Both had to do with sun-spots, the phenomena of which had fascinated him from childhood. The first of these, in which he had as associates Gale and Adams,

resulted in an explanation of many of the chief features of the sun-spot spectrum and the discovery of temperature classes among the lines of the principal elements, and thus laid much of the observational basis for later applications of the theory of ionization and the analysis of spectra according to energy levels in the atom. The results of the investigation when applied by others to stellar spectra led through a few simple steps to the discovery of the relationship between luminosity and certain spectral criteria, and thus to the spectroscopic method of deriving stellar distances.

Comparisons of sun-spot spectra with those of the sun's disk had shown that among the lines of the same element some were greatly strengthened, others but slightly affected, and still others were weakened. These last were recognized as lines which are much more prominent in the spectrum of the electric spark than in that of the arc when studied in laboratory sources and were called "enhanced" lines. They are now known to be due to the radiation of the ionized atom. Since the most reasonable hypothesis was that the temperature of spots is below that of the general surface of the sun, a laboratory investigation was begun in which the temperature of the light-source could be varied and the corresponding spectral changes studied. It at once appeared that the lines fell into distinct classes, some of the lines being greatly strengthened at low temperatures while others were little affected. A comparison showed that just the lines most strengthened at low temperatures in the electric arc were most strengthened in sun-spots, and that the correspondence between the laboratory and the sun-spot behavior was essentially complete.

The investigation was continued, and the discovery of molecular bands in the spectrum of sun-spots confirmed beyond question the assumption of the lower temperatures in spots. On the laboratory side, the study by A. S. King of the spectra of numerous elements under the controlled temperature conditions of the electric furnace led to the accurate classification of the lines according to temperature and provided a collection of data of the utmost value for physical studies of stellar spectra and the quantitative analysis of the spectra of the elements which developed in later years. At about the same time laboratory

studies by Gale and Adams showed that the intensity of the enhanced lines is greatly increased by reduction of pressure in the gas surrounding the source of light, and the suggestion was made that this effect might account for the abnormally high intensity of such lines in the spectrum of the solar chromosphere where the density is extremely low. These results thus anticipated some of the conclusions of the ionization theory developed later by Saha. It is probably not too much to say that this investigation of sun-spot and laboratory spectra, originating with Hale, with all its ramifications and applications, has been one of the most fruitful in its results of any in the field of astrophysics and spectroscopy.

One very important feature of the sun-spot spectrum was left unexplained by the study of the influence of temperature upon spectral lines. This was the widening and in some cases doubling of numerous lines, first discovered by Young, measured systematically by visual methods by some English observers, and studied in detail by W. M. Mitchell at the Allegheny Observatory. The explanation of the phenomenon as one of self-reversal did not fully satisfy Hale and he reverted to the problem when sufficiently powerful spectroscopic equipment became available at Mount Wilson. The successive steps by which he was led to what was perhaps his most brilliant discovery, that of magnetism in the sun, form a most interesting story.

Observations with the spectroheliograph had in past years been limited to the use of H and K and one or two of the blue hydrogen lines because of the lack of sensitiveness of photographic emulsions to light of longer wave-length. The development of red-sensitive dyes and their application to plates by R. J. Wallace in 1907 made it possible to use the  $H\alpha$  line with the spectroheliograph. This led at once to results of great interest. Bright flocculi were found to be much more numerous and intense than on photographs taken with the blue hydrogen line  $H\delta$ , large prominences were photographed as dark areas in projection against the sun's disk, and the intensity and contrast of the images were greatly superior to anything obtained previously. Most important of all, these photographs showed clearly the existence of curved and radial structure in the flocculi surrounding spots of such a character as to indicate

definitely that spots are centers of attraction for the surrounding hydrogen atmosphere and are associated with cyclonic whirls or vortices. The frequent similarity of the distribution of the hydrogen flocculi around spots to that of iron filings in a magnetic field at once caught Hale's attention, and in his well-known paper on Solar Vortices he referred to the possibility of the circular polarization in opposite directions of the components of the double lines in spot spectra and of his intention of undertaking the necessary observations. In June 1908 when suitable spots were available he obtained satisfactory photographs using a rhomb and Nicol prism and proved conclusively the presence of a magnetic field.

Problems growing out of this remarkable discovery occupied Hale for many years. He undertook extensive and systematic measurements of the strength of field in all available spots; studied the behavior of lines in the spectrum as the spots moved toward the sun's edge and the direction of observation changed from one parallel to one at right angles to the lines of force; he discovered the fact that the preceding and the following spots of a normal group have polarities of opposite sign, and that the spots in each hemisphere of the sun are predominantly of one sign but opposite to that in the other hemisphere; and he carried on with much ingenuity experiments on artificial vortices in an attempt to clarify his conceptions of the vortices in the sun. Extensive investigations of the Zeeman effect, conducted in the Pasadena physical laboratory by King and Babcock, provided much material to supplement and check at many points the solar results. Hale also designed at this time the 150-foot tower telescope, completed in 1910, to provide a larger solar image and a spectrograph 75 feet in length. Most of the solar spectroscopic investigations were then transferred to this instrument, and as the sun-spot activity declined Hale commenced with it the most difficult and exacting research of his life, the attempt to detect a general magnetic field in the sun.

During these years the permanent staff and equipment of the Observatory had increased greatly. The 60-inch telescope required several observers and a program of photometric investigation was planned and put into operation by F. H. Seares, who joined the staff in 1909; Ritchey and Pease carried on

direct photography and the spectrographic work was shared by Adams, Babcock, Pease and one or two general assistants. In the physical laboratory King and Babcock were actively engaged in spectroscopic studies, and in the solar department C. E. St. John had begun his long series of investigations, in some of which he was associated with Hale. Many of the problems of stellar spectroscopy had always made a strong appeal to Hale but unfortunately he had found through repeated experience that the strain of night observing placed too severe a burden upon his far from rugged health. Although this was a great disappointment to him, he retained a keen and active interest in stellar observations and contributed greatly to plans for the extension of such work and to the design of apparatus for increasing the efficiency of the observations.

A notable event in the history of the Observatory at this time was the gift of Mr. John D. Hooker in 1906 for the purchase of a 100-inch glass disk for a very large reflecting telescope. Although a telescope of such size necessarily involved many unknown factors, Hale did not hesitate to support the project enthusiastically, feeling confident that the successive problems could be solved satisfactorily as they were encountered. The casting of the disk by the French Plate Glass Company met many difficulties and was long delayed, and the disk when it finally arrived appeared of somewhat questionable quality. A series of tests, however, removed most of these doubts and after some further delay optical work was commenced. Meanwhile a visit by Andrew Carnegie to Mount Wilson in March 1910 had resulted in a further large gift to the Carnegie Institution and the expression of a desire that the 100-inch telescope be carried to completion. The construction of the larger portions of the mounting was delayed by the outbreak of the Great War and the first tests of the finished instrument were not made until October 1917. It was a great satisfaction to Hale, who had been in ill health for several years, to be present on this occasion and to find that his hopes regarding the value of this great telescope were to be so fully realized.

Soon after his discovery of the magnetic field in sun-spots Hale had his first intimation of the severe nervous breakdown which occurred early in 1910. It was doubtless occasioned partly

by the extremely intense way in which he always worked, both mentally and physically, and partly by the strain and the sense of responsibility he felt with regard to the justification of the new observatory. The new problem of the design and construction of the 100-inch Hooker telescope also placed a severe burden upon him, and the meeting of the International Union for Cooperation in Solar Research at Mount Wilson in the summer of 1910 and contact with a large number of visiting astronomers had a stimulating but probably somewhat injurious effect. He was obliged to give up all active investigation for more than a year, and a similar attack recurred in 1913. From this time until the end of his life he was at no time completely free from more or less severe effects of brain congestion, sometimes accompanied by considerable pain and frequently by depression with occasional confusion of thought. These symptoms were greatly aggravated when he devoted himself to the scientific researches in which he was most interested, and after repeated courageous attempts he gradually turned to less exciting and exacting work. Life had become a problem of recognizing his physical limitations and of utilizing his abilities to a considerable extent in other fields.

Two investigations of later years, however, should be noted in connection with any description of Hale's scientific activities. Both grew out of his discovery of a magnetic field in sun-spots. The first was the strong evidence produced for the existence of a general magnetic field in the sun; and the second was the remarkable discovery of the reversal of the direction of sun-spot polarities in the two solar hemispheres with the cycle of spot activity. These investigations were constantly in Hale's mind, and up to his last years he devoted his gradually failing energies to the exacting measurements required in the study of the general field and to the invention of new methods of detecting the minute quantities involved.

The observations on the general field were commenced in the spring of 1912 when sun-spot activity had reached a low stage and the disturbing effects of spot fields were relatively slight. The apparatus used consisted of a compound quarter-wave plate and large Nicol prism mounted just above the slit of the 75-foot spectrograph of the 150-foot tower telescope. The

photographs, taken usually at the most favorable solar latitude of  $45^\circ$ , showed a series of spectra which passed through successive strips of the quarter-wave plate and hence should reveal slight displacements of the spectral lines owing to the alternate extinction of their red and violet components if a magnetic field were present. It was soon found that any such field must be relatively weak, not more than 25 to 50 gaussses instead of 2000 gaussses or more as in an average sun-spot. A careful selection was made of lines of suitable quality which were known to have large Zeeman separations, and these lines were measured on a large number of plates with the aid of a machine provided with a tipping glass plate designed by Hale for measuring minute displacements. An immense amount of data was obtained in this way, chiefly through the measures of A. van Maanen, and was discussed by Hale and by Seares in publications appearing in 1913. The results appeared fairly conclusive in indicating a general field of the order of 50 gaussses at the sun's pole, although as was pointed out by Hale some observers who measured a few of the plates obtained almost negative results.

A short supplementary series of photographs taken in September 1916 helped to confirm the results, and when combined with the earlier observations gave a value of 31.4 days for the period of revolution of the magnetic axis about the sun's axis of rotation.

The evidence for the existence of the magnetic field may be summarized briefly as follows: (1) the reversal of the algebraic signs of the displacements with the inversion of the quarter-wave plate, or the rotation of a half-wave plate used in its place; (2) the variation of the displacements with solar latitude in close agreement with the theoretical variation for a uniformly magnetized sphere; (3) the existence of lines which show no displacements by the general field, whereas systematic errors should affect all the lines measured; (4) the displacement-curves over a long period of time showing changes of form which indicate an inclination of the magnetic axis to the axis of rotation; (5) the correlation between field-strength and magnetic separation of the lines measured.

These results appeared very convincing but another series of photographs was obtained near the following sun-spot minimum

of 1922-23. Although these plates were measured by several observers and by numerous independent methods, including that of tracings with the microphotometer, no absolutely definite conclusion could be drawn from the results. It seems significant, however, that in no case did the mean measures show displacements opposed to those to be expected from a magnetic field as great as those favorable to its presence, a result inconsistent with the assumption of purely accidental errors in the absence of a field. Since the quantities involved are exceedingly small, about 0.001 angstrom at a maximum, it is evident that, in spite of the great amount of observational material, turbulence and slight convection currents in the sun's atmosphere might possibly influence the results appreciably. Or it is even conceivable that the general field may be subject to considerable variations. The final proof of a general magnetic field in the sun may come from quite other sources, such, for example, as its effect upon radiation passing near the sun; but there can be little doubt that in the course of this long and trying investigation Hale showed a degree of skill, conscientiousness, and patience which could serve as a model in scientific research.

The discovery of the reversal of sun-spot polarities with the spot cycle resulted from the systematic study of spots planned and put into operation by Hale soon after his discovery of their magnetic fields. It is well known that at the beginning of a cycle of activity new spots first appear at relatively high solar latitudes and gradually approach the equator as the cycle develops. At sun-spot maximum nearly all spots are found in two zones of latitude a few degrees north and south of the equator, and these zones persist as the activity wanes. After the minimum is passed spots are seen in two noticeably different areas, those of the new cycle beginning to appear in high latitudes, but the occasional spots of the old cycle still remaining in low latitudes.

During the interval 1908-1912 near the time of sun-spot minimum Hale had found that in the great majority of cases the standard strip of the compound quarter-wave plate in his polarizing apparatus transmitted the violet component of preceding spots of bipolar groups in the northern hemisphere and the red component of similar groups in the southern hemisphere.

In the case of the following spots of bipolar groups the transmission was just the opposite. After the spot minimum which occurred in December 1912 new spots began to appear in high latitudes and observations at once showed a reversal of polarity: that is, where the transmission for a preceding spot had been violet in the northern hemisphere and red in the southern, it was now red in the northern hemisphere and violet in the southern. Similarly the polarities of the following spots of bipolar groups had reversed.

These results were so surprising that although Hale and his collaborators published their observations in 1918 the desire for full confirmation and additional data led Hale to delay further discussion until after the next sun-spot minimum of 1922. In a publication (with S. B. Nicholson) written in June, 1925, Hale formulates his well-known polarity law based upon 1735 groups of spots:

"The sun-spots of a new  $11\frac{1}{2}$ -year cycle, which appear in high latitudes after a minimum of solar activity, are of opposite magnetic polarity in the northern and southern hemispheres. As the cycle progresses the mean latitude of the spots in each hemisphere steadily decreases, but their polarity remains unchanged. The high-latitude spots of the next  $11\frac{1}{2}$ -year cycle, which begin to develop more than a year before the last low-latitude spots of the preceding cycle have ceased to appear, are of opposite magnetic polarity."

The 23-year interval between the successive appearances in high latitudes of spots of the same magnetic polarity Hale designated as the magnetic sun-spot period, and of its fundamental importance in solar theory there can be no question.

No one can look back upon Hale's scientific life and accomplishments without a feeling of astonishment that in a single individual there could be combined so many qualities essential to the finest type of productive research. Apparently born with a passion for physical science, he entered upon his work with an enthusiasm and joyous optimism that no difficulties could seriously affect. Extraordinarily resourceful and with a wide and accurate knowledge of physical methods and instruments, he could always devise the apparatus to undertake a complicated observational problem. His reading and travel kept him closely

in touch with developments in technical and applied science and his mind was always engaged in plans for utilizing such developments in his own researches. Hale made constant use of tentative hypotheses to aid in the interpretation of his scientific results, but he treated them merely as tools and no one was more ready to discard any hypothesis immediately when it came into conflict with observational facts. Few scientists have lived who demanded such complete and incontestable evidence for any conclusion he drew from his results ; and few have had in greater measure the rare capacity for the selection of problems which proved fruitful and productive in the highest degree.

The outbreak of the World War found Hale in Pasadena temporarily incapacitated by illness. Although his relations with many German scientists had been most cordial, he had always been suspicious of the plans and aims of their arrogant military rulers, and these views were strengthened during a short visit to Germany in 1913. The war, therefore, came to him as no surprise. Soon after the sinking of the *Lusitania* he suggested to the President of the National Academy of Sciences that the services of the Academy be offered to the President of the United States to assist in preparations for possible war. This question was later raised before the Academy Council and acted upon by unanimous vote of the whole Academy. A committee was appointed to call upon President Wilson, who proved to be favorable to the plan and asked the Academy to begin organization at once. Hale served as chairman of the organizing committee and from 1916 until the spring of 1919 spent most of his time in Washington, making only occasional visits to his home in Pasadena.

This was the origin of the National Research Council of which Hale was the first chairman. As a special organization, established under the national charter of the Academy, it had a wide membership and included representatives of governmental scientific and technical agencies, and of scientific, medical, and engineering bodies throughout the United States. Its value was such that at the close of the war on Hale's recommendation it was perpetuated through an Executive Order of President Wilson. The Council was reorganized on a peace basis, and through a gift from the Carnegie Corporation, which Hale was largely

instrumental in securing, a building for the Academy and Research Council and funds for administration became available.

Hale was by nature an internationalist in his views, and his wide acquaintance with European men of science and the obvious opportunities for cooperation in research increased his interest in international organizations planned to promote this aim. Reference has already been made to the International Union for Cooperation in Solar Research, the first plans for which were made by Hale at the St. Louis Exposition in 1904. Its formal organization took place at Oxford in September of the following year. As Foreign Secretary of the National Academy during several years following 1910 Hale had attended meetings of the International Association of Academies but was considerably depressed by its extreme conservatism and lack of activity in promoting research. Later the bitter feelings occasioned by the war made it clear that this organization could no longer function successfully. Accordingly late in 1918 Hale assisted in organizing two meetings, one at the Royal Society in London and the other at the Paris Academy of Sciences, at which the problem was discussed and the decision was reached to establish the International Research Council, later the International Council of Scientific Unions. Originally having a membership of only 12 countries it now includes 40 nations, and the individual International Unions in various branches of science organized by the Council have been most active and effective agencies in furthering international research.

Perhaps no single activity of Hale's life serves as a better illustration of his foresight and breadth of view in educational and cultural developments than the part he took in the establishment of the California Institute of Technology. During the early years of the Mount Wilson Observatory his assistance was asked in planning the future policy of Throop Institute, a Pasadena school with small endowment which had been attempting to meet local needs in education through elementary courses, an art school, manual training, and other scattered facilities. Hale at once advised a concentration of activities upon science and a few branches of engineering together with adequate instruction in the humanities. From his own experience in a school of technology he had found that students usually ac-

quired but little interest in literature, history, and other humanistic subjects, and also that fundamental science was too often neglected in the training for engineering.

Hale's policy was adopted and gradually the transformation of the school into an institution of advanced standing was put into effect. At first students were few in number, but rigorous standards were maintained, endowment funds were raised, and one by one departments were established and organized. Hale succeeded in interesting his friend A. A. Noyes of the Massachusetts Institute of Technology in the development of the new school, and Noyes, who had organized the Research Laboratory of Physical Chemistry at the Institute and had served for two years as Acting President, agreed to spend each winter quarter in Pasadena. Soon after the war he joined permanently the staff of the California Institute of Technology, as the new institution was now named, and his influence and that of Hale went far toward defining its policy in the years to come.

In 1920 the first president, J. A. B. Scherer, was obliged to resign because of ill health, and the critical question of his successor arose. Greatly to the delight of Hale and Noyes they succeeded in prevailing upon R. A. Millikan to accept the directorship of the Norman Bridge Laboratory of Physics and the chairmanship of the Executive Council, the governing body directing the affairs of the Institute. By Millikan's wish the administration of the Institute was conducted no longer through a president but through this Council, composed in part of trustees and in part of representatives of the faculty.

The rapid rise of the California Institute under Millikan's leadership to a very high place among the educational institutions of the country is so well known as to require no comment. Hale continued as a trustee and a member of the Executive Council until the time of his death, and the great success of the institution to which he had given so much thought and which represented so many of his ideals in education was of immense comfort to him throughout these darker years of his life.

Hale's acquaintance with Henry E. Huntington began not long after the establishment of the Mount Wilson Observatory. At this time Huntington had already begun collecting rare books and paintings and the eventual disposal of his collections had

commenced to occupy his thoughts. Accordingly he asked Hale's advice upon the subject and gradually Hale evolved the plan which later developed into the establishment of the endowed Huntington Library and Art Gallery with its magnificent treasures of books, manuscripts, and paintings. Two considerations were fundamental in Hale's mind: first, that if these collections were to be accessible to the public their management should be in the hands of a board of competent trustees free from political control; and secondly, that the wealth of material contained in the incunabula, manuscripts, and rare books should be made available for use by scholars for literary and historical research. The conception of a research institution mainly along humanistic lines operating in association with the research organizations in physical science already established in Pasadena appealed greatly to Hale and he lost no opportunity of presenting his views to Mr. Huntington. Late in 1925 a statement of policy following closely Hale's suggestions was approved and signed by Mr. Huntington, and on February 8, 1926, a supplemental trust indenture was executed. Early in 1927 a Director of Research was appointed, and the new institution was established with an adequate endowment. In the quality of its collections and the admirable use which is made of them the Huntington Library and Art Gallery ranks high among the great libraries of the world.

These brief accounts of a few of Hale's major activities outside of his immediate scientific work serve merely to illustrate the wide extent of his interests and accomplishments. In a thousand ways, in his community and nationally and internationally, his fertile mind was constantly engaged in assisting or organizing cultural and scientific agencies, in aiding scholars, and in promoting the free interchange of thought throughout the world. He served upon the Committee on Intellectual Cooperation of the League of Nations in 1922; aided in establishing the National Research Fellowships awarded annually by the Rockefeller Boards; and was active in developing in many directions the efficiency of the National Academy and the National Research Council. In his own community he took part in every important cultural movement, and it was upon his suggestion that the City Planning Commission was organ-

ized which has contributed so greatly to the attractiveness of the municipal buildings of Pasadena. The award in 1927 of the Noble Medal for Civic Service by the City of Pasadena was in recognition of some of Hale's many contributions to the welfare of the city in which he lived.

*1922-1938*

A severe nervous breakdown in 1921 had obliged Hale to take a long rest. In the summer of 1922 he went to England and travelled through portions of western Europe finally reaching Egypt where he spent most of the winter. Here he renewed his friendship of long standing with J. H. Breasted, the archaeologist, and together they visited the tomb of Tutenkhamon before the contents had been removed. Breasted was at this time deciphering the seals on the still unopened doorway of the sarcophagus chamber. Hale and Breasted also visited Florence where Hale found the opportunity of observing the sun and planets with one of Galileo's original telescopes. Unfortunately the rest produced no great improvement in Hale's health, and he reached the decision that he must resign the directorship of the Mount Wilson Observatory. In view of the opportunities for research at an institution developed in accordance with his plans of long standing, and his great love of the mountain top and its natural surroundings, this decision was a most difficult and painful one to make. He faced it courageously, however, in the hope that under conditions of less responsibility and less contact with some of the most exciting aspects of research he might still find it possible to accomplish a part of the work he had in mind. He returned to Pasadena and resigned the directorship in the summer of 1923, retaining at the request of his associates the title of Honorary Director and such part in questions of general policy at the Observatory as his health would permit him to take.

Faced with the problem of adjusting his life and work to his weakened physical condition Hale decided to build a small well-equipped solar laboratory where he could work on problems of the sun as his strength permitted, and could meet from time to time his individual associates under conditions affording quiet and freedom from excitement. A building was erected

by Hale at his own expense not far from the grounds of the California Institute of Technology and equipped with a vertical coelostat and spectrograph built in the instrument shop of the Observatory. Through the use of the Cassegrain design with convex mirrors, images of the sun up to 16 inches in diameter could be obtained with the telescope. The spectrograph was placed in an underground pit, as in the case of the Mount Wilson tower telescopes. With his usual generosity Hale made over to the Carnegie Institution as a part of the Mount Wilson equipment both the plant and the instruments, merely asking to be allowed to use them as long as he retained his ability to observe.

Before the Solar Laboratory was completed Hale returned to a problem which had interested him in earlier years. This was to render visible to the eye the bright and dark flocculi which are photographed with the spectroheliograph. His skill and ingenuity were never more clearly shown than in his invention of the spectrohelioscope—a special type of spectroscope with an oscillating slit or rotating prism above the slit. With this instrument the whole surface of the sun can be scanned within a few minutes, and short-lived eruptions which might otherwise pass unrecorded can be detected and measured. The spectrohelioscope has proved extremely valuable for observations of the solar activity, and, through a cooperative plan devised by Hale, the sun is now under nearly continuous observation with instruments of this type located in suitable zones of longitude over the earth's surface.

Observations with the spectrohelioscope, many attempts to improve methods for detecting the general magnetic field of the sun, and studies of sun-spot polarities occupied the limited time which Hale found it physically possible to devote to such work during the years following his retirement from the Observatory. But heavy responsibilities awaited him in connection with a new project, the last and in some respects the greatest of the many with which he had been associated during his life. This was the plan for the 200-inch telescope, and although Hale realized the strain which would be placed upon his frail health by the many problems and decisions which the project involved he accepted the burden gladly. It represented a further great addition to the series of instrumental develop-

ments which he had contributed so often to the progress of astronomy.

Before returning to Pasadena in 1923 Hale had devoted considerable time while in London to writing a number of articles for *Scribner's Magazine* on various developments in modern astronomy and astrophysics. He was remarkably successful, for he combined a simplicity and clearness of statement with an unusual ability to select the subjects of especial interest to the general reader. In 1927 he was asked by the editor of *Harper's Magazine* to write a similar article and Hale selected the title "The Possibilities of Large Telescopes." This article appeared in April 1928. Before its publication, however, Hale had written to Wickliffe Rose, President of the General Education Board, with whom he was well acquainted and whose enthusiastic interest in scientific research he well knew, regarding the possibility of financial support for the project of a very large telescope. Hale suggested a joint arrangement with the Carnegie Institution of Washington or with the National Academy of Sciences.

Dr. Rose expressed a keen interest in the plan and after an interview with Hale in New York visited Pasadena and Mount Wilson. As a result of this visit and after several discussions in New York with trustees of the Rockefeller and Carnegie foundations, and in Pasadena with the groups at the California Institute of Technology and the Mount Wilson Observatory, Dr. Rose recommended a grant for the construction of a 200-inch telescope. This recommendation received favorable action by the Executive Committee of the International Education Board on October 18, 1928. After assurance had been received from the President of the Carnegie Institution of Washington of the willingness of the Institution to assist and cooperate in the undertaking the telescope was given to the California Institute of Technology on this cooperative basis. An Observatory Council consisting of Hale as Chairman, Millikan, Noyes, and Robinson was designated by the Board of Trustees of the California Institute to carry out the entire project. At a somewhat later date Adams was added to its membership. J. A. Anderson of the Mount Wilson staff was made Executive Officer of the Coun-

cil, and several committees were appointed to consider various phases of the project.

Hale continued as Chairman of the Observatory Council until his death, and it was only within the last few months of his life that he found it impossible to give personal attention to the problems under consideration. At that time Max Mason became Vice-Chairman and took over Hale's responsibilities. Up to the very end, however, Hale retained the deepest interest in the progress of the telescope and the construction of the buildings on Palomar Mountain, and his contributions to the success of the project were fundamental. Decisions upon the type of glass to be used in the great disk as well as its structure and support, the form of mounting to be adopted, the location of the telescope, and the auxiliary instruments and buildings to be constructed, were all made under his guidance and advice. Although he could not live to see the completion of his last and greatest telescope, he was able to realize that the vital problems were solved and that its success was with all reasonable certainty fully assured.

The condition of Hale's health had been slowly growing worse since 1927 and after 1934 additional complications set in which reduced his strength rapidly. He died on February 21, 1938, a few months before his seventieth birthday. Although even his closest friends had seen him but rarely during the last year of his life, it was most difficult to realize the fact of his death, such was the strength and vividness of his remarkable personality.

Few American scientists had the degree of recognition at home and abroad which Hale received. He was awarded the Janssen medal of the Paris Academy of Sciences in 1894, at the age of 26; Rumford medal in 1902; Draper medal, 1903; Royal Astronomical Society medal, 1904; Bruce medal, 1916; Janssen medal, 1917; Galileo medal, 1920; Actonian prize, 1921; Cresson medal, 1926; Franklin medal, 1927; Holland Society medal, 1931; Ives medal, 1935; and the Copley medal of the Royal Society, 1932. In addition to membership in many scientific societies in the United States and Europe he was a foreign member of the Royal Society of London, the Royal Society of Edinburgh, the Royal Society of Dublin, Société Hollandaise des Sciences, Philosophical Society of Cambridge, Accademia dei

Lincei, Amsterdam Academy of Sciences, Norwegian Academy of Sciences, Swedish Academy of Sciences, Russian Academy of Sciences, and the Société Imperiale des Naturalistes of Moscow. He was also a foreign associate or honorary member of the Institut de France (Academy of Sciences), Italian Society of Sciences, Royal Academy of Belgium, Royal Astronomical Society, Academy of Athens, Vienna Academy of Sciences, Royal Society of Upsala, the Academies of Catania, Genoa, and Turin, Société de Physique of Geneva, Royal Institution of London, London Physical Society, Société Française de Physique, and of the Franklin Institute.

No brief statement of a few of Hale's major activities can give any adequate picture of the life of this remarkably gifted and many-sided man. His sensitive mind like a delicate musical instrument of many strings responded to every contact with nature or the touch of poetry or music or art. Trained as a physicist and engineer, he was also a humanist and classicist with a profound appreciation of the contributions of the older civilizations to the beauty and values of life. The rare combination of an extraordinary clear and analytical mind with a far-reaching and yet controlled imagination explains better than anything else the extent of his influence upon the development of science during his years of activity and the permanence of his contributions to the intellectual life of the period.

No one could be associated with Hale without falling at once under the charm of his vivid and inspiring personality. With every characteristic of a brilliant and highly-cultured mind he retained a thoughtfulness and sympathy for others and a modesty which endeared him to every colleague. This aspect of a man of great human qualities could not be better illustrated than by the closing words of some unpublished biographical notes written in the later years of his life:

"Whatever I have accomplished has been chiefly due to the friendly support and cooperation of others. The only qualities I can claim are an intense interest in the objects I have sought and a willingness to work to the full limit of my strength to secure their accomplishment. Fortunate beyond words in my family, my friends, and my colleagues, I have received far too much of the credit which more truly belongs to them."

# KEY TO ABBREVIATIONS

- Amer. Assoc. Proc.—American Association for the Advancement of Science, Proceedings.  
 Amer. Jour. Sci.—American Journal of Science.  
 Ap. J.—Astrophysical Journal.  
 Astron. and Astrophys.—Astronomy and Astrophysics.  
 Astron. Nachr.—Astronomische Nachrichten.  
 Atlantic Mo.—Atlantic Monthly.  
 B. A. A. S.—British Association for the Advancement of Science.  
 Bull. Amer. Inst. Mining Eng.—Bulletin, American Institute of Mining Engineers.  
 Bull. Soc. fr. de Physique—Bulletin, Société française de physique.  
 Bull. Soc. Arts—Bulletin, Society of Arts.  
 Carnegie Inst. Yearbook—Carnegie Institution of Washington Yearbook.  
 Harper's Mag.—Harper's Magazine.  
 Jour. Amer. Soc. Mech. Eng.—Journal, American Society of Mechanical Engineers.  
 Jour. B. A. A.—Journal, British Astronomical Association.  
 Jour. de physique—Journal de physique.  
 Jour. Franklin Inst.—Journal, Franklin Institute.  
 Mem. Spett. It.—Memorie della Società degli spettroscopisti italiani.  
 M. N.—Monthly Notices, Royal Astronomical Society.  
 Mt. W. Comm.—Mt. Wilson Communications.  
 Mt. W. Contr.—Mt. Wilson Contributions.  
 Nat. Acad. Sci. Rept.—National Academy of Sciences, Report.  
 Nat. Hist.—Natural History.  
 Nat. Res. Council Reprint & Circular Series—National Research Council, Reprint and Circular Series.  
 Obs.—Observatory.  
 Philos. Soc.—Philosophical Society.  
 Phys. Zeits.—Physikalische Zeitschrift.  
 Pop. Astron.—Popular Astronomy.  
 Pop. Sci. Mo.—Popular Science Monthly.  
 Proc. Amer. Inst. Elec. Eng.—Proceedings, American Institute of Electrical Engineers.  
 Proc. Amer. Philos. Soc.—Proceedings, American Philosophical Society.  
 Proc. Nat. Acad. Sci.—Proceedings, National Academy of Sciences.  
 Proc. R. Inst.—Proceedings, Royal Institution.  
 Proc. R. Soc.—Proceedings, Royal Society of London.  
 Pub. A. A. S.—Publications, American Astronomical Society.  
 Pub. A. S. P.—Publications, Astronomical Society of the Pacific.  
 Pub. Astron. and Astrophys. Soc.—Publications, Astronomical and Astrophysical Society of America.  
 Pub. Yerkes Obs.—Publications, Yerkes Observatory.  
 Research narratives—Popular research narratives, collected by the Engineering Foundation, N. Y.

- Riv. di Astron.—Rivista di Astronomia.  
 Sci. Amer.—Scientific American.  
 Sci. Amer. Supp.—Scientific American Supplement.  
 Scribner's Mag.—Scribner's Magazine.  
 Sid. Mgr.—Sidereal Messenger.  
 Smiths. Rept.—Smithsonian Institution Report.  
 Smiths. Coll.—Smithsonian Institution Miscellaneous Collections.  
 Sunset Mag.—Sunset Magazine.  
 Tech Engineering News—Monthly of Massachusetts Institute of Technology.  
 Tech. Quar.—Technology Quarterly, Massachusetts Institute of Technology.  
 Tech. Rev.—Technology Review.  
 Terr. Mag.—Terrestrial Magnetism.  
 Trans. Int. Astron. Union—Transactions, International Astronomical Union.  
 Trans. Int. Union Coop. Sol. Res.—Transactions, International Union for Co-operation in Solar Research.  
 Trans. R. Canadian Inst.—Transactions, Royal Canadian Institute.  
 Vo. Mag.—Vo-Mag, (Vocational Magazine), Pasadena (Calif.) Junior College.  
 Yerkes Obs. Bull.—Yerkes Observatory, Bulletin.

## BIBLIOGRAPHY OF GEORGE ELLERY HALE

1889

- Stellar photography. The Beacon (Chicago), March, pp. 54-57.  
 The new astronomy. The Beacon (Chicago), July-Aug., pp. 164-165, 176-178.

1890

- Note on solar prominence photography. Astron. Nachr., 126, pp. 81-82.  
 Photography of the solar prominences. Tech. Quar., 3, pp. 310-316.

1891

- Photographic investigation of solar prominences and their spectra. Amer. Jour. Sci., 42, pp. 160-166.  
 Note on solar prominence photography. Astron. Nachr., 127, pp. 211-214.  
 On results obtained in photographing solar prominences and their spectra. Astron. Nachr., 128, pp. 109-110.  
 The ultra-violet spectrum of the solar prominences. Read at Cardiff meeting, British Association for the Advancement of Science, 1891; Report B. A. A. S., pp. 557-558; Amer. Jour. Sci., 42, pp. 459-467; Astron. and Astrophys., 11, pp. 50-59, 1892.  
 On results obtained in photographing solar prominences and their spectra. Mem. Spett. It., 20, pp. 140-141.

- The ultra-violet spectrum of the solar prominences. *Mem. Spett. It.*, 20, pp. 154-160.
- The Kenwood Physical Observatory. *Pub. A. S. P.*, 3, 30-34; *Sid. Mgr.*, 10, pp. 321-323.
- Organization of the Chicago Section of the Astronomical Society of the Pacific. *Pub. A. S. P.*, 3, pp. 79-82.
- The system of the stars, by A. M. Clerke (Review). *Pub. A. S. P.*, 3, pp. 180-194.
- Researches on the magnesia fluting in connection with the spectra of the nebulae. *Sid. Mgr.*, 10, pp. 23-30.
- Pending problems in spectroscopy. *Sid. Mgr.*, 10, pp. 89-94.
- Photography and the invisible solar prominences. *Sid. Mgr.*, 10, pp. 257-264.

1892

- The spectroheliograph of the Kenwood Astrophysical Observatory, Chicago, and results obtained in the study of the sun. *Amer. Assoc. Proc.*, pp. 55-56.
- The Astro-physical Journal. *Astron. and Astrophys.*, 11, pp. 17-22.
- Recent results in solar prominence photography. *Astron. and Astrophys.*, 11, pp. 70-78.
- Note on recent solar investigations. *Astron. and Astrophys.*, 11, p. 159.
- Progress in solar photography at the Kenwood Observatory. *Astron. and Astrophys.*, 11, p. 235.
- The aurora of February 13, 1892. *Astron. and Astrophys.*, 11, pp. 237-239.
- The eruptive prominence of July 9, 1891. *Astron. and Astrophys.*, 11, pp. 240-241.
- The spectra of sun-spots and the photosphere. *Astron. and Astrophys.*, 11, pp. 241-243.
- The astronomical exhibit at the World's Columbian Exposition. *Astron. and Astrophys.*, 11, pp. 305-307; *Pub. A. S. P.*, 4, pp. 123-125.
- Spectroscopic observations of the great sun-spot group of February, 1892. *Astron. and Astrophys.*, 11, pp. 310-314.
- Solar photography at the Kenwood Astrophysical Observatory. *Astron. and Astrophys.*, 11, pp. 407-417; *Mem. Spett. It.*, 21, pp. 68-74.
- The ultra-violet spectrum of the solar prominences, II. *Astron. and Astrophys.*, 11, p. 602.
- Photographs of solar phenomena obtained with the spectroheliograph of the Kenwood Astrophysical Observatory. *Astron. and Astrophys.*, 11, pp. 603-604.
- Photographs of the occultation of Mars by the moon (July 11, 1892), made at Kenwood Astrophysical Observatory. *Astron. and Astrophys.*, 11, pp. 610-611.
- A remarkable solar disturbance. *Astron. and Astrophys.*, 11, pp. 611-613.
- Magnetic disturbances and auroras. *Astron. and Astrophys.*, 11, p. 617.
- The ultra-violet spectrum of solar prominences, II (second note). *Astron. and Astrophys.*, 11, p. 618.

- Chicago Academy of Sciences. *Astron. and Astrophys.*, *11*, pp. 635-636, 846-847.
- Progress of solar photography at the Kenwood Observatory. *Astron. and Astrophys.*, *11*, p. 741.
- The Yerkes Observatory of The University of Chicago. *Astron. and Astrophys.*, *11*, pp. 790-793; *Pub. A. S. P.*, *4*, pp. 250-252.
- Some results and conclusions derived from a photographic study of the sun. *Astron. and Astrophys.*, *11*, pp. 811-815.
- The ultra-violet spectrum of the solar prominences, III. *Astron. and Astrophys.*, *11*, pp. 821-822.
- On the condition of the sun's surface in June and July, 1892, as compared with the record of terrestrial magnetism. *Astron. and Astrophys.*, *11*, pp. 917-925.
- Photographies de la chromosphère, des protubérances, et des facules solaires, à l'Observatoire d'Astronomie physique de Kenwood, Chicago. *Comptes rendus*, *115*, pp. 106-109.
- Photographs of solar faculae and prominences. *Knowledge*, *15* (n. s., 7), p. 169. (Photographs only.)
- Recherches sur l'atmosphère solaire. *Mem. Spett. It.*, *21*, pp. 77-78; *Comptes rendus*, *114*, pp. 1406-1407.

1893

- Chicago Academy of Sciences, Section of Mathematics and Astronomy. *Astron. and Astrophys.*, *12*, pp. 94-95, 285, 477-478.
- On the probability of chance coincidence of solar and terrestrial phenomena. *Astron. and Astrophys.*, *12*, pp. 167-169.
- The spectroheliograph. *Astron. and Astrophys.*, *12*, pp. 241-257.
- Photography of the solar corona without an eclipse. *Astron. and Astrophys.*, *12*, pp. 260-262.
- Photography of the solar corona without an eclipse (Note). *Astron. and Astrophys.*, *12*, pp. 364-365.
- Spectroscopic notes from the Kenwood Observatory. *Astron. and Astrophys.*, *12*, pp. 450-455.
- Attempt to photograph the corona from Pike's Peak. *Astron. and Astrophys.*, *12*, p. 653.
- Spectrum of comet *b* 1893. *Astron. and Astrophys.*, *12*, p. 653.
- The Congress of Mathematics, Astronomy and Astrophysics (at the Columbian Exposition). *Astron. and Astrophys.*, *12*, pp. 640-641.
- Astronomical exhibits at The Columbian Exposition. *Astron. and Astrophys.*, *12*, pp. 641-645.
- The Congress of Mathematics, Astronomy, and Astrophysics, Section of Astronomy and Astrophysics. *Astron. and Astrophys.*, *12*, pp. 746-749.
- Photography of the corona without an eclipse. *Astron. and Astrophys.*, *12*, p. 751.
- Les raies H et K dans le spectre des facules solaires. *Comptes rendus*, *116*, pp. 170-172.

- La probabilité de coïncidence entre les phénomènes terrestres et solaires. *Comptes rendus*, *116*, pp. 240-243.
- Photographie de la couronne solaire, en dehors des éclipses totales. *Comptes rendus*, *116*, pp. 623-625.
- Méthode spectro-photographique pour l'étude de la couronne solaire. *Comptes rendus*, *116*, pp. 865-866.
- The sun as a bright-line star. *Knowledge*, *16* (n. s., 8), pp. 192, 232-233.
- A remarkable solar disturbance. *Mem. Spett. It.*, *21*, pp. 106-107.
- Photographies de la chromosphère, des protubérances et des facules solaires, à l'Observatoire d'Astronomie physique de Kenwood (Chicago). *Mem. Spett. It.*, *21*, pp. 108-109.
- The ultra-violet spectrum of the solar prominences. *Mem. Spett. It.*, *21*, pp. 160-161.
- Some results and conclusions derived from a photographic study of the sun. *Mem. Spett. It.*, *21*, pp. 165-168.
- Photographie de la couronne solaire en dehors des éclipses totales. *Mem. Spett. It.*, *22*, pp. 66-67.
- Fotografie solari. *Mem. Spett. It.*, *22*, p. 89.
- The solar faculae. *Mem. Spett. It.*, *22*, pp. 198-204.
- Remarks on solar phenomena before the Royal Astronomical Society. *Obs.*, *16*, pp. 395-396.
- Some recent photographic investigations of the sun. *Tech. Quar.*, *6*, pp. 262-272.

1894

- Monsieur Cornu's spectroscopic investigations. *Astron. and Astrophys.*, *13*, pp. 57-58.
- Spectroscopy at Stonyhurst College Observatory. *Astron. and Astrophys.*, *13*, pp. 58-59.
- New nomenclature for the hydrogen series. *Astron. and Astrophys.*, *13*, p. 63.
- Spectroheliograph for the Yerkes telescope. *Astron. and Astrophys.*, *13*, pp. 64-66.
- The solar faculae. *Astron. and Astrophys.*, *13*, pp. 113-122.
- The spectroscopic investigations of Mr. George Higgs. *Astron. and Astrophys.*, *13*, pp. 151-153.
- On some attempts to photograph the solar corona without an eclipse. *Astron. and Astrophys.*, *13*, pp. 662-687.
- A collection of astronomical photographs. *Astron. and Astrophys.*, *13*, pp. 693-695.
- Dr. Huggins' 1888 photograph of the spectrum of the great nebula in Orion. *Astron. and Astrophys.*, *13*, p. 696.
- The Astrophysical Journal. *Astron. and Astrophys.*, *13*, pp. 831-835.
- The solar faculae. *Knowledge*, *17* (n. s., 9), pp. 18-22.
- On the mounting of large telescopes. *Knowledge*, *17* (n. s., 9), pp. 145-146.

Sur les facules solaires. *Mem. Spett. It.*, 23, pp. 110-111; *Comptes rendus*, 118, pp. 1175-1177.

Ueber die photographische Erforschung der Sonne. *Verhandlungen der Phys. Gesellschaft*, Berlin, 13, pp. 1-3.

1895

Preliminary note on the D<sub>3</sub> line in the spectrum of the chromosphere.

*Astron. Nachr.*, 138, pp. 227-230; *Mem. Spett. It.*, 24, pp. 117-119.

The Astrophysical Journal. *Ap. J.*, 1, pp. 80-84.

Spectro-bolographic investigations at the Smithsonian Astrophysical Observatory. *Ap. J.*, 1, pp. 162-166.

Arthur Cowper Ranyard. *Ap. J.*, 1, pp. 168-169.

Publications of the Lick Observatory, volume 3 (Review). *Ap. J.*, 1, pp. 180-188.

On a new method of mapping the solar corona without an eclipse. *Ap. J.*, 1, pp. 318-334.

A large eruptive prominence. *Ap. J.*, 1, pp. 433-434.

On a photographic method of determining the visibility of interference fringes in spectroscopic measurements. *Ap. J.*, 1, pp. 435-438.

Note on the exposure required in photographing the solar corona without an eclipse. *Ap. J.*, 1, pp. 438-439.

Terrestrial helium (?) (Review). *Ap. J.*, 1, pp. 439-441.

Notes on Schmidt's theory of the sun. *Ap. J.*, 2, pp. 73-74.

Note on the Yerkes Observatory. *Ap. J.*, 2, pp. 74-76; *Pub. A. S. P.*, 7, pp. 225-226.

On the presence of helium in clèveite. *Ap. J.*, 2, pp. 76-77.

Note on the Huggins method of photographing the solar corona without an eclipse. *Ap. J.*, 2, p. 77.

Note on the D<sub>3</sub> line in the spectrum of the chromosphere. *Ap. J.*, 2, pp. 165-166.

On the wave-length of the D<sub>3</sub> line in the spectrum of the chromosphere. *Ap. J.*, 2, pp. 384-385.

On some attempts to photograph the solar corona without an eclipse. The expedition to Mount Etna. *Mem. Spett. It.*, 24, pp. 31-35.

1896

Action of the Editorial Board of the Astrophysical Journal with regard to standards in astrophysics and spectroscopy. *Ap. J.*, 3, pp. 1-3.

Note on the application of Messrs. Jewell, Humphreys and Mohler's results to certain problems of astrophysics. *Ap. J.*, 3, pp. 156-161.

Yerkes Observatory, University of Chicago (Yerkes Obs. Bull., No. 1). *Ap. J.*, 3, pp. 215-219; *Astron. Nachr.*, 140, pp. 305-306.

The sun, by C. A. Young (Review). *Ap. J.*, 3, pp. 235-240.

Observations des protubérances solaires faites à l'Observatoire d'Odessa, by A. Kononowitch and others (Review). *Ap. J.*, 3, p. 241.

The effect of a total eclipse of the sun on the visibility of the solar prominences. *Ap. J.*, 3, pp. 374-387.

The modern spectroscope. XIX. The objective spectroscope. (With F. L. O. Wadsworth.) *Ap. J.*, 4, pp. 54-78.

Observations de l'éclipse totale du soleil du 16 Avril, 1893, by H. Deslandres (Review). *Ap. J.*, 4, pp. 160-166.

1897

On the comparative value of refracting and reflecting telescopes for astrophysical investigations. *Ap. J.*, 5, pp. 119-131.

Note on the Ranyard mounting for reflecting telescopes. *Ap. J.*, 5, pp. 148-149.

The Yerkes Observatory of the University of Chicago: I. Selection of the site. *Ap. J.*, 5, pp. 164-180.

Note on a form of spectroheliograph suggested by Mr. H. F. Newall. *Ap. J.*, 5, pp. 211-213.

Award of the Gold Medal of the Royal Astronomical Society to Professor Barnard. *Ap. J.*, 5, pp. 215-216.

Total eclipse of the sun, April 16, 1893, by J. N. Lockyer (Review). *Ap. J.*, 5, pp. 220-226.

The Yerkes Observatory of the University of Chicago: II. The building and minor instruments. *Ap. J.*, 5, pp. 254-267.

The Yerkes Observatory of the University of Chicago: III. The instruments and optical shops, and the power house. *Ap. J.*, 5, pp. 310-317.

Analyse spectrale directe des minéraux, by A. de Gramont (Review). *Ap. J.*, 5, pp. 362-363.

The Yerkes Observatory of the University of Chicago: IV. The forty-inch telescope, dome, and rising floor. *Ap. J.*, 6, pp. 37-47.

Dedication of the Yerkes Observatory. *Ap. J.*, 6, p. 58.

Alvan Graham Clark. *Ap. J.*, 6, pp. 137-138.

Completion of the 40-inch telescope (Yerkes Obs. Bull., No. 2). *Ap. J.*, 6, pp. 147-153.

Note on the relative frequency of the H and K lines in the spectrum of the chromosphere. *Ap. J.*, 6, pp. 157-158.

Dedication of the Yerkes Observatory (Yerkes Obs. Bull., No. 3). *Ap. J.*, 6, pp. 262-263.

The aim of the Yerkes Observatory. Read at Yerkes meeting, Astronomical and Astrophysical Society of America, 1897; *Ap. J.*, 6, pp. 310-321.

The dedication of the Yerkes Observatory. *Ap. J.*, 6, pp. 353-362.

Note on the level of sun-spots. *Ap. J.*, 6, p. 366.

On the presence of carbon in the chromosphere. *Ap. J.*, 6, pp. 412-414.

Researches in solar physics. Read at Yerkes meeting, Astronomical and Astrophysical Society of America, 1897; *Pub. Astron. and Astrophys. Soc.*, 1, p. 39, 1910.

1898

The total solar eclipse. *Ap. J.*, 7, pp. 304-306.

The new Director of the Lick Observatory. *Ap. J.*, 7, p. 310.

- Astronomical and Physical Conference at the Harvard College Observatory. *Ap. J.*, 8, pp. 54-55.
- Election of Edwin B. Frost as Professor of Astrophysics at the Yerkes Observatory. *Ap. J.*, 8, p. 59.
- The Harvard Conference. *Ap. J.*, 8, pp. 193-198.
- On the spectra of stars of Secchi's Fourth Type. *Ap. J.*, 8, pp. 237-240.
- First annual report of the Director of the Yerkes Observatory for the year ending Sept. 30, 1898. 11 pp.
- On the spectra of stars of Secchi's Fourth Type (Abstract). Read at Harvard meeting, Astronomical and Astrophysical Society of America, 1898. *Pub. Astron. and Astrophys. Soc.*, 1, pp. 53-55, 1910.
- On the spectra of stars of Secchi's Fourth Type. *Science*, n. s., 8, pp. 455-456.
- The function of large telescopes; address given at Fifth Annual Reception, New York Academy of Science. *Smiths. Rept.* 1898, pp. 123-137; *Science*, n.s., 7, pp. 650-662.

1899

- Parallax of the Andromeda nebula (Yerkes Obs. Bull., No. 6). *Ap. J.*, 9, pp. 184-185.
- The spectrum of Saturn's rings (Yerkes Obs. Bull., No. 6). *Ap. J.*, 9, pp. 185-186.
- Spectra of stars of Secchi's Fourth Type (Yerkes Obs. Bull., No. 7). *Ap. J.*, 9, pp. 271-272.
- Opportunities for students and research assistants at the Yerkes Observatory (Yerkes Obs. Bull., No. 8). *Ap. J.*, 9, pp. 272-273.
- Comparison of stellar spectra of the Third and Fourth Types (Yerkes Obs. Bull., No. 9). *Ap. J.*, 9, pp. 273-274.
- Period and elongation distance of the Fifth Satellite of Jupiter (Yerkes Obs. Bull., No. 10). *Ap. J.*, 9, pp. 358-360.
- Heat radiation of the stars (Yerkes Obs. Bull., No. 11). *Ap. J.*, 9, pp. 360-362; *Pop. Astron.*, 7, pp. 503-504.
- The new Allegheny Observatory. *Ap. J.*, 9, p. 362.
- On the spectra of stars of Secchi's Fourth Type. I. (With Ferdinand Ellerman.) *Ap. J.*, 10, pp. 87-112.
- The Third Conference of Astronomers and Astrophysicists. *Ap. J.*, 10, pp. 211-220.
- Some new forms of spectroheliographs. Read at Yerkes meeting, Astronomical and Astrophysical Society of America, 1899; *Ap. J.*, 10, pp. 288-289 (Yerkes Obs. Bull., No. 12); *Science*, 10, p. 795; (Abstract) *Pub. Astron. and Astrophys. Soc.*, 1, pp. 86-87, 1910.
- Annales de l'Observatoire d'Astronomie Physique de Paris*, Tome premier (Review). *Ap. J.*, 10, pp. 369-374.
- The Indian eclipse, by E. W. Maunder (Review). *Ap. J.*, 10, pp. 374-376.
- Second annual report of the Director of the Yerkes Observatory for the year ending June 30, 1899. 5 pp.

- Carbon in the chromosphere. Read at Yerkes meeting, Astronomical and Astrophysical Society of America, 1899; (Abstract) *Pub. Astron. and Astrophys. Soc.*, 1, p. 86, 1910.
- Comparison of stellar spectra of the third and fourth types. Read at Yerkes meeting, Astronomical and Astrophysical Society of America, 1899; (Abstract) *Pub. Astron. and Astrophys. Soc.*, 1, p. 87, 1910.
- Carbon in the chromosphere. *Ap. J.*, n.s., 10, pp. 287-288 (Yerkes Obs. Bull., No. 12); *Science*, 10, pp. 794-795.
- Comparison of stellar spectra of the third and fourth types. *Science*, n.s., 10, p. 795.
- Report of the Board of Visitors to the U. S. Naval Observatory, October 2, 1899. Govt. Printing Office, Washington, D. C. 77 pp. (Sec. I): *Ap. J.*, 10, pp. 292-299; *Pop. Astron.*, 7, pp. 459-465; *Science*, n.s., 10, pp. 747-751.

1900

- Solar eclipse problems. *Ap. J.* 11, pp. 47-66.
- Plans of American eclipse parties. *Ap. J.*, 11, pp. 314-321.
- Variable star observations with the 12-inch and 40-inch refractors (Yerkes Obs. Bull., No. 13). *Ap. J.*, 12, pp. 52-54.
- Observations of the total solar eclipse of May 28, 1900, at Wadesboro, N. C. (Yerkes Obs. Bull., No. 14). *Ap. J.*, 12, pp. 80-89.
- James Edward Keeler. *Ap. J.*, 12, p. 102; *Science*, n.s., 12, p. 353.
- Photographs of the cluster M 13 Hercules with the 40-inch visual telescope. (Yerkes Obs. Bull., No. 15.) *Ap. J.*, pp. 161-164.
- An atlas of representative stellar spectra, by Sir William and Lady Huggins (Review). *Ap. J.*, 12, pp. 291-297.
- On some attempts to detect the solar corona in full sunlight with a bolometer. *Ap. J.*, 12, pp. 372-375.
- Appointment of Professor W. W. Campbell as Director of the Lick Observatory. *Ap. J.*, 12, pp. 381-382.
- Observing stations for the total eclipse, May 28, 1900. *Pop. Astron.*, 8, p. 295.

1901

- The new star in Perseus (Yerkes Obs. Bull., No. 16). *Ap. J.*, 13, pp. 173-176; *Pop. Astron.*, 9, pp. 264-267; *Science*, n.s., 13, pp. 514-515.
- Changes in the spectrum of Nova Persei (Yerkes Obs. Bull., No. 17). *Ap. J.*, 13, pp. 238-240.
- Annals of the Astrophysical Observatory of the Smithsonian Institution, vol. 1 (Review). *Ap. J.*, 13, pp. 280-288.
- Latitude and longitude of the Yerkes Observatory (Yerkes Obs. Bull., No. 18). *Ap. J.*, 14, pp. 146-148.
- A remarkable disturbance of the sun's reversing layer (Abstract). Read at Washington meeting, Astronomical and Astrophysical Society of America, 1901; *Pub. Astron. and Astrophys. Soc.*, 1, p. 177, 1910.

1902

- Note on the spark spectrum of iron in liquids and in air at high pressures. *Ap. J.*, 15, pp. 132-135.
- Selective absorption as a function of wave-length. *Ap. J.*, 15, pp. 227-228.
- Handbuch der astronomischen Instrumentenkunde, by L. Ambrohn (Review). *Ap. J.*, 15, pp. 347-349.
- Solar research at the Yerkes Observatory. *Ap. J.*, 16, pp. 211-233.
- Report of the Director of the Yerkes Observatory for the period July 1, 1899, to June 30, 1902. 32 pp. Also in *Pub. A. S. P.*, 15, pp. 99-110, 1903.
- Report of Advisory Committee on Astronomy. (With others.) Carnegie Inst. Yearbook, No. 1, pp. 87-93.
- General plan for furthering special researches in astronomy. Carnegie Inst. Yearbook, No. 1, pp. 94-104.
- Progress and present state of certain departments of astrophysical research. Carnegie Inst. Yearbook, No. 1, pp. 128-144.
- Stellar evolution in the light of recent research; address delivered before Minnesota Chapter, Society of Sigma Xi, June 5, 1901; *Pop. Sci. Mo.*, 60, pp. 291-313; *Smiths. Rept.* 1902, pp. 149-163.
- The spectra of stars of Secchi's Fourth Type. (With F. Ellerman and J. A. Parkhurst.) Read at Washington meeting, Astronomical and Astrophysical Society of America, 1902; (Abstract) *Pub. Astron. and Astrophys. Soc.*, 1, pp. 183-184, 1910.
- A remarkable disturbance of the sun's reversing layer. *Science*, n.s., 15, p. 297.

1903

- Second note on the spark spectrum of iron in liquids and compressed gases. (With Norton A. Kent.) *Ap. J.*, 17, pp. 154-160.
- The new star in Gemini (Yerkes Obs. Bull., No. 19). *Ap. J.*, 17, pp. 300-305.
- The Snow horizontal telescope. *Ap. J.*, 17, p. 314.
- The spectra of stars of Secchi's Fourth Type. (With Ferdinand Ellerman and J. A. Parkhurst.) Decennial publication, University of Chicago, ser. 2, 8, pp. 251-385; *Pub. Yerkes Obs.*, 2 (Decennial papers), pp. 251-385, 1904.
- The Rumford spectroheliograph of the Yerkes Observatory. (With Ferdinand Ellerman.) *Pub. Yerkes Obs.*, 3 (Pt. 1), pp. 1-26.
- Abstract of report on grant to Yerkes Observatory. Carnegie Inst. Yearbook, No. 2, p. xx.
- Report of Committee on Southern and Solar Observatories. (With Lewis Boss and W. W. Campbell.) Carnegie Inst. Yearbook, No. 2, pp. 5-70.
- The spectra of stars of Secchi's Fourth Type. (With Ferdinand Ellerman and J. A. Parkhurst.) *Science*, n.s., 17, p. 324.

1904

- The development of a new method of research; address delivered before University of Chicago Chapter, Society of Sigma Xi, Nov. 23, 1903; *Pop. Sci. Mo.*, 65, pp. 5-26.
- Calcium and hydrogen flocculi. (With Ferdinand Ellerman.) *Ap. J.*, 19, pp. 41-52.
- Handbuch der Spectroscopie (vol. 2), by H. Kayser (Review). *Ap. J.*, 19, pp. 296-300.
- An expedition for solar research. *Ap. J.*, 19, pp. 385-386.
- Cooperation in solar research. *Ap. J.*, 20, pp. 306-312; *Science*, n.s., 20, pp. 930-932; *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 11-19, 1906; (Italian translation) *Cooperazione internazionale nelle ricerche solari. Mem. Spett. It.*, 33, pp. 209-211.
- Report of the Director of the Yerkes Observatory for the period July 1, 1902, to June 30, 1904. 8 pp.
- Abstract of report on grant for researches at Yerkes Observatory. *Carnegie Inst. Yearbook*, No. 3, pp. 88-90.
- Report on the Solar Observatory on Mount Wilson. *Carnegie Inst. Yearbook*, No. 3, pp. 94-95.
- Experiments on the use of fused quartz for the construction of optical mirrors. *Carnegie Inst. Yearbook*, No. 3, pp. 127-128.
- A study of the conditions for solar research at Mount Wilson. *Carnegie Inst. Yearbook*, No. 3, pp. 155-177; *Ap. J.*, 21, pp. 124-150, 1905; *Mt. W. Contr.*, No. 1.
- Some examples of solar work in which co-operation would be desirable. *Mem. Spett. It.*, 33, p. 44.

1905

- The Solar Observatory of the Carnegie Institution of Washington. *Ap. J.*, 21, pp. 151-172; *Mt. W. Contr.*, No. 2.
- The work of the Rumford spectroheliograph. *Ap. J.*, 21, pp. 261-270.
- Report of Director of the Solar Observatory, Mount Wilson, California. *Carnegie Inst. Yearbook*, No. 4, pp. 56-77.
- The development of a new observatory (Mount Wilson). *Pub. A. S. P.*, 17, pp. 41-52.
- Tests of the Snow telescope. *Pub. A. S. P.*, 17, pp. 100-101.
- Smithsonian expedition to Mount Wilson. *Pub. A. S. P.*, 17, pp. 108-109.
- Water system for the Solar Observatory. *Pub. A. S. P.*, 17, p. 109.

1906

- Circular sent out by the Committee on Solar Research of the National Academy of Sciences (1904). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 1-4.
- Standards of wave-length (1904). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 7-8.
- Work with the spectroheliograph (1904). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 76-79.

- Spectra of sun-spots (1905). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 118-119.
- Solar radiation (1905). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 103-104.
- Spectroheliograph (1905). *Trans. Int. Union Coop. Sol. Res.*, 1, pp. 107-111.
- A program of solar research. *Ap. J.*, 23, pp. 1-5; *Mt. W. Contr.*, No. 3.
- Some tests of the Snow telescope. *Ap. J.*, 23, pp. 6-10; *Mt. W. Contr.*, No. 4.
- Photographic observations of the spectra of sun-spots. (With W. S. Adams.) *Ap. J.*, 23, pp. 11-44; *Mt. W. Contr.*, No. 5.
- The five-foot spectroheliograph of the Solar Observatory. (With Ferdinand Ellerman.) *Ap. J.*, 23, pp. 54-63; *Mt. W. Contr.*, No. 7.
- Reply to recent statements by M. Deslandres. *Ap. J.*, 23, pp. 92-93.
- Sun-spot lines in the spectra of red stars. (With W. S. Adams.) *Ap. J.*, 23, pp. 400-405; *Mt. W. Contr.*, No. 8.
- Latitude and longitude of the Solar Observatory. *Ap. J.*, 24, pp. 129-132; *Mt. W. Contr.*, No. 9.
- The spectroscopic laboratory of the Solar Observatory. *Ap. J.*, 24, pp. 61-68; *Mt. W. Contr.*, No. 10.
- Preliminary paper on the cause of the characteristic phenomena of sun-spot spectra. (With W. S. Adams and H. G. Gale.) *Ap. J.*, 24, pp. 185-213; *Mt. W. Contr.*, No. 11; (Italian translation) *Comunicazione preliminare sulla causa dei fenomeni caratteristici degli spettri delle macchie solari. Mem. Spett. It.*, 35, pp. 208-215.
- A 100-inch mirror for the Solar Observatory. *Ap. J.*, 24, pp. 214-218; *Mt. W. Contr.*, No. 13; *Pop. Astron.*, 14, pp. 557-560; *Pub. A. S. P.*, 18, pp. 272-274.
- Report on the Solar Observatory, Mount Wilson, California. *Carnegie Inst. Yearbook*, No. 5, pp. 60-86; *Pub. A. S. P.*, 19, pp. 100-104, 1907.
- A program of solar research. *Pub. A. S. P.*, 18, pp. 59-62.
- Some tests of the Snow telescope. *Pub. A. S. P.*, 18, pp. 73-76.
- Photographic observations of the spectra of sun-spots. (With W. S. Adams.) *Pub. A. S. P.*, 18, pp. 76-77.
- Report of the Solar Observatory of the Carnegie Institution of Washington, 1905. *Pub. A. S. P.*, 18, pp. 156-164.
- Height of the hydrogen flocculi. *Pub. A. S. P.*, 18, pp. 221-222.
- Identification of faint lines in the spectra of sun-spots. *Pub. A. S. P.*, 18, pp. 222-223.
- Organization of the computing division of the Solar Observatory. *Pub. A. S. P.*, 18, p. 223.
- The five-foot reflector of the Solar Observatory. *Pub. A. S. P.*, 18, p. 224.
- New members of the staff of the Solar Observatory. *Pub. A. S. P.*, 18, p. 228.
- Smithsonian expedition to Mount Wilson. *Pub. A. S. P.*, 18, p. 229.
- Note on the level of sun-spots. *Pub. A. S. P.*, 18, pp. 250-251.
- A road to Mount Wilson. *Pub. A. S. P.*, 18, pp. 251-252.

- On the cause of the characteristic phenomena of sun-spot spectra. (With W. S. Adams and H. G. Gale.) Pub. A. S. P., 18, pp. 268-270.  
 Character and cause of sun-spot spectra. (Abstract) Nature, 75, pp. 113-114.  
 Opportunities for solar research. Read at New York meeting, Astronomical and Astrophysical Society of America; Pub. Astron. and Astrophys. Soc., 1, pp. 287-292, 1910.

1907

- A vertical coelostat telescope. Ap. J., 25, pp. 68-74; Mt. W. Contr., No. 14.  
 Second paper on the cause of the characteristic phenomena of sun-spot spectra. (With W. S. Adams.) Ap. J., 25, pp. 75-94; Mt. W. Contr., No. 15.  
 The heliomicrometer. Ap. J., 25, pp. 293-299; Mt. W. Contr., No. 16.  
 A photographic comparison of the spectra of the limb and the center of the sun. (With W. S. Adams.) Ap. J., 25, pp. 300-310; Mt. W. Contr., No. 17.  
 Some new applications of the spectroheliograph. Ap. J., 25, pp. 311-314; Mt. W. Contr., No. 18.  
 Mount Wilson Solar Observatory (annual report). Carnegie Inst. Year-book, No. 6, 134-152; Pub. A. S. P., 20, pp. 107-116, 1908.  
 Some opportunities for astronomical work with inexpensive apparatus. Lecture delivered at Royal Astronomical Society, London, June 26, 1907. M. N., 68, pp. 64-80, 1907; Pop. Astron., 16, pp. 74-80, 167-174, 438-442, 1908; Jour. B. A. A., 18, 1907-1908, Appendix, pp. 1-18; Smiths. Rept. 1907, pp. 267-285; (German tr.) Einige Gelegenheiten zu Astronomischen Arbeiten mit wohlfeilen Apparaten. Phys. Zeits., 10, pp. 707-719, 1909.  
 Solar Observatory work (extract from the Annual report, Mt. Wilson Obs., 1907). Pop. Astron., 15, pp. 563-566.  
 Opportunities for solar research. Read before the Astronomical and Astrophysical Society, New York meeting, 1906; Pub. A. S. P., 19, pp. 27-33; Science, n. s., 25, pp. 613-617.  
 Spectra of the limb and center of the sun. (With W. S. Adams.) Pub. A. S. P., 19, pp. 239-240.  
 Preliminary photographic map of the sun-spot spectrum. Pub. A. S. P., 19, p. 240.  
 Recent progress in the construction work of the Solar Observatory. Pub. A. S. P., 19, pp. 243-244.  
 Professor Julius's visit to Mount Wilson. Pub. A. S. P., 19, p. 244.  
 New appointments to the staff of the Mount Wilson Solar Observatory. Pub. A. S. P., 19, pp. 244-245.  
 A plea for the imaginative element in technical education. 15 pp.  
 The spectrum of the high potential discharge between metallic electrodes in liquids and in gases at high pressures. (With Norton A. Kent.) Pub. Yerkes Obs., 3, (Pt. 2), pp. 27-66.

1908

- Remarques sur le rapport de la commission sur les travaux à faire avec le spectrohéliographe (1907). *Trans. Int. Union Coop. Sol. Res.*, 2, pp. 56-62.
- Report of Committee on Work with the Spectroheliograph (1907). *Trans. Int. Union Coop. Sol. Res.*, 2, pp. 181-190.
- The measurement of solar photographs made with the spectroheliograph (1907). *Trans. Int. Union Coop. Sol. Res.*, 2, pp. 191-198.
- The Mount Wilson photographic map (1907). *Trans. Int. Union Coop. Sol. Res.*, 2, pp. 216-217.
- The tower telescope of the Mount Wilson Solar Observatory. *Ap. J.*, 27, pp. 204-212; *Mt. W. Contr.*, No. 23.
- Preliminary note on the rotation of the sun as determined from the motions of the hydrogen flocculi. *Ap. J.*, 27, pp. 219-229; *Mt. W. Contr.*, No. 25.
- Solar vortices. *Ap. J.*, 28, pp. 100-116; *Mt. W. Contr.*, No. 26; *Pub. A. S. P.*, 20, pp. 203-219.
- The Pasadena Laboratory of the Mount Wilson Solar Observatory. *Ap. J.*, 28, pp. 244-249; *Mt. W. Contr.*, No. 27.
- On the probable existence of a magnetic field in sun-spots. *Ap. J.*, 28, pp. 315-343; *Mt. W. Contr.*, No. 30.
- Mount Wilson Solar Observatory (Annual report). *Carnegie Inst. Year-book*, No. 7, pp. 146-157; *Pub. A. S. P.*, 21, pp. 146-159, 1909.
- Rotation period of the sun as determined from the motions of the calcium flocculi. (With Philip Fox.) *Carnegie Inst. Pub. No. 93*; *Wash. Carnegie Inst.* 54 pp.
- The study of stellar evolution; an account of some recent methods of astrophysical research (Decennial publication of University of Chicago, ser. 2, v. 10). *Chicago, Univ. Chicago Press.* 252 pp.
- The relative advantages of various forms of telescopes for solar research; read at April meeting, American Philosophical Society, April 25, 1908. (Not printed.)
- A comparative study of the spectra of the limb and center of the sun. (With W. S. Adams.) *Pub. A. S. P.*, 20, pp. 27-28.
- Some tests of the vertical coelostat or "tower" telescope of the Mount Wilson Solar Observatory. *Pub. A. S. P.*, 20, pp. 35-36.
- Solar vortices and the Zeeman effect. *Pub. A. S. P.*, 20, pp. 220-224.
- The Zeeman effect in the sun. *Pub. A. S. P.*, 20, pp. 287-288.
- The hydrogen flocculi, as photographed with the  $H\alpha$  and  $H\delta$  lines. *Mem. Spett. It.*, 37, pp. 99-100; (Fr. tr.) *Les flocculi de l'hydrogène photographiés avec les raies  $H\alpha$  et  $H\delta$ . Comptes rendus*, 146, pp. 1251-1254.
- Note on Sutherland's article. *Terr. Mag.*, 13, p. 158.
- On the probable existence of a magnetic field in sun-spots. (Author's abstract.) *Terr. Mag.*, 13, pp. 159-160.

1909

- Photography of the "flash" spectrum without an eclipse. (With W. S. Adams.) *Ap. J.*, 30, pp. 222-230; *Mt. W. Contr.*, No. 41.
- Solar Observatory (Annual report). *Carnegie Inst. Yearbook*, No. 8, pp. 159-180.
- Solar vortices and magnetic fields (published also under the titles: Recent solar research; *Sur les champs magnétiques des taches solaires*; *Sui campi magnetici delle macchie solari*). Address given at dedication of Frank Brackett Observatory, Claremont, Calif., Nov. 20, 1908; *Société française de Physique* (Paris), April 23, 1909; *Royal Institution of Great Britain*, May 14, 1909; *Soc. Italiana di Fisica* (Rome), June 2, 1909; *Proc. R. Inst.*, 19, pp. 615-630; *Pop. Astron.*, 17, pp. 14-19; *Obs.*, 32, pp. 311-315; *Nature*, 82, pp. 20-23, 50-53; *Jour. de physique* (4), 8, pp. 461-475; *Bull. Soc. fr. de Physique*, pp. 118-132; *Riv. di Astron.*, 3, October, 1909; *Sci. Amer. Supp.*, 69, pp. 213-215, 236-237, 1910.
- Remarques relatives à la communication de M. Deslandres. *Comptes rendus*, 148, p. 1025.
- Reconnaissance des couches supérieures du calcium et de l'hydrogène dans l'atmosphère solaire et des mêmes filaments noirs dans les deux couches. *Comptes rendus*, 148, pp. 1025-1026.
- On the nature of the hydrogen flocculi on the sun; address before *Royal Society of London*, June 17, 1909. *M. N.*, 69, pp. 616-617.
- Celestial photographs taken at Mount Wilson Observatory, California. *M. N.*, 70, pp. 175-176.
- Recent work of the Mount Wilson Observatory. *Science*, n. s., 29, pp. 220-222.
- Some recent contributions to our knowledge of the sun; Hamilton lecture of the Smithsonian Institution, delivered April 22, 1908. *Smiths. Coll.*, 52, pp. 331-360.
- In Galileo's footsteps. (With Alexander McAdie and Stanley Du Bois.) *Sunset Mag.*, 22, pp. 133-143.
- Solar cyclones and magnetic fields; address before *Society of Arts, Massachusetts Institute of Technology*. *Bull. Soc. Arts*, April, 1909. 9 pp.
- Note on the magnetic field in sun-spots. *Pub. A.S.P.*, 21, pp. 205-208.

1910

- The work of the Mount Wilson Observatory; lecture before *American Philosophical Society*, April 22, 1910. (Never printed.)
- Notes on the solar magnetic fields and related phenomena. *Pub. A.S.P.*, 22, pp. 63-78.
- Classification of the hydrogen ( $H\alpha$ ) flocculi. *Pub. A.S.P.*, 22, pp. 142-143.
- Asymmetrical lines in sun-spot spectra. *Pub. A.S.P.*, 22, p. 145.
- Sixth interim report on Mars: Addendum. *Jour. B.A.A.*, 20, pp. 191-192.

On the nature of the hydrogen flocculi and their structure at different levels in the solar atmosphere. (With Ferdinand Ellerman.) *Proc. R. Soc., Ser. A*, 83, pp. 177-188.

Address at opening of Fourth Conference of the International Union for Co-operation in Solar Research (1910). *Trans. Int. Union Coop. Sol. Res.*, 3, pp. 11-26, 1911.

Great solar storms. *Literary Digest*, 41, pp. 978-979.

I canali di Marte. *Riv. di Astron.*, 4, pp. 113-116.

1911

Spectroheliograph; article in *Encyclopaedia Britannica*, 11th ed., 25, pp. 618-619.

1912

Mount Wilson Solar Observatory (Annual report). *Carnegie Inst. Yearbook*, No. 11, pp. 172-213.

Polarization phenomena of coelostat telescopes. *Pub. A.S.P.*, 24, pp. 73-75.

A method of testing optical glass. *Pub. A.S.P.*, 24, pp. 128-129.

The 150-foot tower telescope of the Mount Wilson Solar Observatory. *Pub. A.S.P.*, 24, pp. 223-226.

A sun-spot hypothesis. *Pub. A.S.P.*, 24, pp. 226-227.

Preliminary note on an attempt to detect the general magnetic field of the sun. Read at Cleveland meeting, *Astronomical and Astrophysical Society of America*, 1912; (Abstract) *Pub. Astron. and Astrophys. Soc.*, 2, 154.

Preliminary note on an attempt to detect the general magnetic field of the sun. *Terr. Mag.*, 17, pp. 173-178.

1913

The work of Sir William Huggins. *Ap. J.*, 37, pp. 145-153.

Preliminary results of an attempt to detect the general magnetic field of the sun. *Ap. J.*, 38, pp. 27-98; *Mt. W. Contr.*, No. 71.

Mount Wilson Solar Observatory (Annual report). *Carnegie Inst. Yearbook*, No. 12, pp. 195-240.

Preliminary note on an attempt to detect the magnetic field of the sun. *Pub. A.S.P.*, 25, pp. 28-33.

The general magnetic field of the sun. (With F. Ellerman and A. van Maanen.) Read at Atlanta meeting, *American Astronomical Society*, 1913; (Abstract) *Pub. A.A.S.*, 3, p. 9, 1918; *Pop. Astron.*, 22, p. 137, 1914.

National academies and the progress of research. I. Work of European academies. *Science*, n.s., 38, pp. 681-698.

Magnetism of celestial bodies. *Sci. Amer. Supp.*, 76, pp. 196-198.

Earth and sun as magnets. Address delivered at the Semi-centennial of the National Academy of Sciences, Washington, D. C., May, 1913. *Smiths. Rept.* 1913, pp. 145-158; *Pop. Sci. Mon.*, 83, pp. 105-124.

The day's work of the Mount Wilson Observatory. *The World's Work*, 25, pp. 568-577.

1914

- Report of the spectroheliograph committee (1913). Trans. Int. Union Coop. Sol. Res., 4, pp. 95-97, 1914.
- Preliminary observations of the Zeeman effect, due to the general magnetic field of the sun (1913). Trans. Int. Union Coop. Sol. Res., 4, pp. 218-221, 1914.
- Mount Wilson Solar Observatory (Annual report). Carnegie Inst. Yearbook, No. 13, pp. 241-284, 1914.
- Solar magnetic phenomena. Read before American Philosophical Society, April 24, 1914; (Abstract) Proc. Amer. Philos. Soc., 53, pp. 251-255.
- The polarity of sun-spots. (With Ferdinand Ellerman.) Read at Evanston meeting, American Astronomical Society, 1914; (Abstract) Pub. A.A.S., 3, pp. 81-82; Pop. Astron., 22, pp. 568-569.
- The Zeeman and Stark effects. Pub. A.S.P., 26, pp. 146-147.
- National academies and the progress of research. II. The first half-century of the National Academy of Sciences. Science, n.s., 39, pp. 189-200.
- National academies and the progress of research. III. The future of the National Academy of Sciences. Read at Baltimore meeting, National Academy of Sciences (1913); Science, n.s., 40, pp. 907-919, 1914; 41, pp. 12-23, 1915.
- The direction of rotation of solar storms. Read before National Academy of Sciences, Chicago meeting (1914); (Abstract) Science, n.s., 40, p. 771.

1915

- Mount Wilson Solar Observatory (Annual report). Carnegie Inst. Yearbook, No. 14, pp. 251-293, 1915.
- An attempt to measure the free electricity in the sun's atmosphere. (With H. D. Babcock.) Proc. Nat. Acad. Sci., 1, pp. 123-127; Mt. W. Comm., No. 3.
- The direction of rotation of sun-spot vortices. Proc. Nat. Acad. Sci., 1, pp. 382-384; Mt. W. Comm., No. 10.
- Some vortex experiments bearing on the nature of sun-spots and flocculi. (With G. P. Luckey.) Proc. Nat. Acad. Sci., 1, pp. 385-389; Mt. W. Comm., No. 11.
- The work of a modern observatory. Pub. A.S.P., 27, pp. 161-178.
- The remodeled 60-foot tower telescope. Pub. A.S.P., 27, pp. 233-235.
- Progress of work on the 100-inch telescope. Pub. A.S.P., 27, pp. 235-236.
- National Academies and the progress of research. IV. The Proceedings of the National Academy as a medium of publication. Science, n.s., 41, pp. 815-817.
- Ten years' work of a mountain observatory (Carnegie Inst. Pub., No. 235). Wash., Carnegie Inst. 99 pp.

1916

- Mount Wilson Solar Observatory (Annual report). Carnegie Inst. Yearbook, No. 15, pp. 227-272.

- Fotografia spettroeliografica (with A. Riccò). *Mem. Spett. It.*, ser. 2a, 5, pp. 32-35.
- Ancora sulla fotografia solare stereoscopica. *Mem. Spett. It.*, ser. 2a, 5, pp. 61-63.
- The National Research Council of the United States. (With others.) *Nature*, 97, pp. 464-465.
- Some reflections on the progress of astrophysics; address delivered at the Semi-Centennial of the Dearborn Observatory. *Pop. Astron.*, 24, pp. 550-558.
- The minute structure of the solar atmosphere. (With Ferdinand Ellerman.) *Proc. Nat. Acad. Sci.*, 2, pp. 102-108; *Mt. W. Comm.*, No. 20.
- Preliminary report of the Organizing Committee to the President of the Academy. *Proc. Nat. Acad. Sci.*, 2, pp. 507-510.
- Work for the amateur astronomer. *Pub. A.S.P.*, 28, pp. 53-61.
- The national value of scientific research. *Tech. Rev.*, 18, pp. 801-817.
- The National Research Council: How it proposes to mobilize the nation's science for industrial progress and military efficiency. *N. Y. Times*, July 26, 1916.
- A National Research Council. *N. Y. Evening Post*, Oct. 9, 1916.

1917

- Baxandall's explanation of an "abnormal" solar spectrum. *Ap. J.*, 46, pp. 291-295; *Mt. W. Contr.*, No. 143.
- Mount Wilson Solar Observatory (Annual report). *Carnegie Inst. Year-book*, No. 16, pp. 199-235.
- The National Research Council: Basis of organization and means of co-operation with state councils of defense. *Jour. Franklin Inst.*, 183, p. 759.
- National Research Council: Research committees in educational institutions. *Proc. Nat. Acad. Sci.*, 3, pp. 223-227.
- The best service of astronomers in time of war. Read at Albany meeting, American Astronomical Society, 1917; *Pub. A.A.S.*, 3, pp. 313-315, 1918; *Pop. Astron.*, 25, pp. 652-655.
- The intellectual inspiration of Paris; introduction to Science and learning in France, pp. 5-17. Published by Society for American Fellowships in French Universities, 1917.
- Second annual report of the National Research Council. *Nat. Acad. Sci. Rept. for 1917*, pp. 46-70.
- How men of science will help in our war. *Scribner's Mag.*, 61, pp. 721-726.
- National Research Council. *Southern Calif. Mag.*, 1, No. 5, p. 6.

1918

- The general magnetic field of the sun: apparent variation of field-strength with level in the solar atmosphere (With F. H. Seares, A. van Maanen, and F. Ellerman.) *Ap. J.*, 47, 206-254; *Mt. W. Contr.*, No. 148.

- Mount Wilson Observatory (Annual report). Carnegie Inst. Yearbook, No. 17, pp. 181-218.
- The national engineering societies and the National Research Council. Jour. Amer. Soc. Mech. Eng., 40, pp. 825-829.
- Industrial research and national welfare. Address delivered under auspices Engineering Foundation, New York, May 28, 1918; (excerpts) Science, n.s., 48, pp. 505-507.
- War activities of the National Research Council; address before Engineering Foundation, May 28, 1918. Proc. Amer. Inst. Elec. Eng., 37, pp. 909-934; Bull. Amer. Inst. Mining Eng., 140, Supp., pp. xxxix-xlvi.
- Third annual report of the National Research Council. Nat. Acad. Sci. Rept. for 1918, pp. 39-112.

1919

- The magnetic polarity of sun-spots. (With F. Ellerman, S. B. Nicholson, and A. H. Joy.) Ap. J., 49, pp. 153-178; Mt. W. Contr., No. 165.
- Mount Wilson Observatory (Annual report). Carnegie Inst. Yearbook, No. 18, pp. 217-264.
- The national importance of scientific and industrial research. (With others.) Nat. Research Council. Bul., 1, pp. 1-7.
- The 100-inch Hooker telescope of the Mount Wilson Observatory. Pop. Astron., 27, pp. 635-636.
- On the nature of sun-spots. (Abstract) Proc. R. Soc., Ser. A., 95, pp. 234-236.
- Preliminary results of a comparative test of the 60-inch and 100-inch telescopes of the Mount Wilson Observatory. Read at Ann Arbor meeting, American Astronomical Society, 1919; (Abstract) Pub. A.A.S., 4, pp. 90-93, 1923; Pop. Astron., 27, pp. 563-566; Pub. A.S.P., 31, pp. 257-260.
- Stereoscopic photographs of the solar atmosphere. (Abstract) Pub. A.S.P., 31, p. 178.
- The responsibilities of the scientist; read at Pasadena meeting, Pacific Division, American Association for the Advancement of Science, 1919; Science, n.s., 50, pp. 143-146; Throop College Bull., 28, pp. 17-24, 1919.
- Co-operation in research; address before Royal Canadian Institute, April 9, 1919. Trans. R. Canadian Inst., 12, pp. 339-347; Science, n.s., 51, pp. 149-155, 1920.
- Science as a source of industrial progress; address before members of the Legislature of the Province of Ontario, Toronto, April 10, 1919. (Never printed.)
- Plan for the promotion of scientific and industrial research by the National Academy of Sciences and the National Research Council. N. Y., Carnegie Corporation, 1919. 32 + xiv pp.

1920

- Mount Wilson Observatory (Annual report). Carnegie Inst. Yearbook, No. 19, pp. 209-265.

- Photographs taken with the 100-inch reflector of the Mount Wilson Observatory. *M.N.*, 80, pp. 240-243.
- Some tests of the 100-inch Hooker telescope. *Nature*, 105, pp. 266-268.
- Note on the 100-inch Hooker telescope. Read at Northampton-South Hadley meeting, American Astronomical Society, 1920; (Abstract) *Pub. A.A.S.*, 4, pp. 161-162, 1923; *Pop. Astron.*, 28, pp. 599-600.
- The Mount Wilson photographic map of the sun-spot spectrum. (With Ferdinand Ellerman.) Read at Northampton-South Hadley meeting, American Astronomical Society, 1920; (Abstract) *Pub. A.A.S.*, 4, pp. 162-164, 1923; *Pop. Astron.*, 28, pp. 600-602; *Pub. A.S.P.*, 32, pp. 272-273.
- Lunar photography with the Hooker telescope. *Pub. A.S.P.*, 32, pp. 112-115.
- The international organization of scientific research. *Internat. Conciliation*, No. 154, Sept., 1920, pp. 431-441.
- Outline of plan for inter-allied research council. No date.
- Introduction; Science and War (Chap. 1); War services of the National Research Council (Chap. 2); The Possibilities of Co-operation in Research (Chap. 22); The International Organization of Research (Chap. 23). In Yerkes, *The New World of Science*.
- The new heavens. *Scribner's Mag.*, 68, pp. 387-402.

1921

- Mount Wilson Observatory (Annual report). *Carnegie Inst. Yearbook*, No. 20, pp. 215-294.
- The angular diameter of  $\alpha$  Orionis. *M. N.*, 81, pp. 166-167.
- Some astrophysical aspects of the Zeeman effect. *Physica*, 1, pp. 250-254.
- The probable absence of a measurable electric field in sun-spots. Read at Middletown meeting, American Astronomical Society, 1921; (abstract) *Pub. A. A. S.*, 4, pp. 262-263; *Pop. Astron.*, 29, pp. 625-626.
- A summary of the year's work at Mount Wilson (1919-1920). *Pub. A. S. P.*, 33, pp. 18-30.
- Note on the combined effect of electric and magnetic fields on the hydrogen spectrum. *Pub. A. S. P.*, 33, pp. 114-116.
- The great sun-spot of May, 1921. (With S. B. Nicholson.) (Abstract) *Pub. A. S. P.*, 33, p. 208.
- A summary of the year's work at Mount Wilson (1920-1921). *Pub. A. S. P.*, 33, pp. 291-312.
- Giant stars. *Scribner's Mag.*, 70, pp. 3-15.
- Cosmic crucibles. *Scribner's Mag.*, 70, pp. 387-401.
- The 100-inch telescope of the Mount Wilson Observatory. *London Times*, July 26, 1921.

1922

- Mount Wilson Observatory (Annual report). *Carnegie Inst. Yearbook*, No. 21, pp. 198-255.
- Invisible sun-spots. *M. N.*, 82, pp. 168-169.

- Invisible sun-spots. *Proc. Nat. Acad. Sci.*, 8, pp. 168-170; *Mt. W. Comm.*, No. 80.
- A fifty-foot interferometer telescope. *Nature*, 110, pp. 482-483.
- A fifty-foot interferometer telescope. Read at Williams Bay meeting, American Astronomical Society, 1922; (Abstract) *Pub. A. A. S.*, 4, pp. 358-359; *Pop. Astron.*, 30, pp. 556-557.
- Note on invisible sun-spots. *Pub. A. S. P.*, 34, pp. 59-61.
- A summary of the year's work at Mount Wilson. (With W. S. Adams.) *Pub. A. S. P.*, 34, pp. 258-274.
- A joint investigation of the constitution of matter and the nature of radiation. Address at dedication of Norman Bridge Laboratory, California Institute of Technology; *Science*, n. s., 55, pp. 332-334.
- Report of President of Commission on Solar Atmosphere. *Trans. Int. Astron. Union*, 1, pp. 31-33.
- The new heavens. N. Y., Scribners. 88 pp.
- The depths of the universe. *Scribner's Mag.*, 71, pp. 689-704.
- A national focus of science and research. *Scribner's Mag.*, 72, pp. 515-530; *Nat. Res. Council, Reprint and Circular Ser.*, No. 39, 16 pp.

1923

- The possibilities of instrumental development. Read at Mount Wilson meeting, American Astronomical Society, 1923; *Pub. A. A. S.*, 5, pp. 54-60, 1927; *Pop. Astron.*, 31, pp. 568-574; *Smiths. Rept.* 1923, pp. 187-193.
- Recent discoveries in Egypt. *Scribner's Mag.*, 74, pp. 34-49.
- The work of an American Orientalist. *Scribner's Mag.*, 74, pp. 392-404.

1924

- Sun-spots as magnets and the periodic reversal of their polarity. *Nature*, 113 (Supp.), pp. 105-112.
- The law of sun-spot polarity. *Proc. Nat. Acad. Sci.*, 10, pp. 53-55; *Mt. W. Comm.*, No. 86.
- The spectrohelioscope. *Proc. Nat. Acad. Sci.*, 10, pp. 361-363; *Mt. W. Comm.*, No. 87; *Nature*, 114, p. 628.
- The depths of the universe. N. Y., Scribner. 98 pp.
- Barnard's dark nebulae. *Scribner's Mag.*, 75, pp. 81-95.
- Sun-spots as magnets. *Scribner's Mag.*, 75, pp. 527-543.
- The periodic reversal of the magnetic polarity of sun-spots. *Tech Engineering News*, 5, pp. 50-51, 84.

1925

- Law of the sun-spot polarity. (With S. B. Nicholson.) *Ap. J.*, 62, pp. 270-300; *Mt. W. Contr.*, No. 300; List of errata. *Ap. J.*, 63, p. 72, 1926.
- History of the Royal Society, 1820-1920, ed. by J. L. E. Dreyer and H. H. Turner (Review). *Ap. J.*, 62, pp. 65-72.
- The spectrohelioscope. *Nature*, 115, p. 50.

A test of the electromagnetic theory of the hydrogen vortices surrounding sun-spots. *Proc. Nat. Acad. Sci.*, 11, pp. 691-696; *Mt. W. Comm.*, No. 95.

Nature of the hydrogen vortices surrounding sun-spots. *Pub. A. S. P.*, 37, pp. 268-270.

The Oriental ancestry of the telescope. *Scribner's Mag.*, 77, pp. 392-404.

Some recollections of Bertram Grosvenor Goodhue. In *Memorial volume*, Bertram Grosvenor Goodhue, Architect and Master of many arts. *Amer. Inst. Architects*, pp. 45-50.

1926

Solar tornadoes. *Nat. Hist.*, 26, pp. 363-373.

Observations with the spectrohelioscope. *Nature*, 118, pp. 420-421.

Tribute to Sir Joseph Thomson. *Nature*, 118, pp. 880-881.

Some new possibilities in solar research. *Nature*, 118 (Supp.), pp. 1-8.

The spectrohelioscope. *Obs.*, 49, p. 372.

Visual observations of the solar atmosphere. *Proc. Nat. Acad. Sci.*, 12, pp. 286-295; *Mt. W. Comm.*, No. 97.

The spectrohelioscope. *Pub. A. S. P.*, 38, pp. 96-100.

Beyond the Milky Way. *Scribner's Mag.*, 80, pp. 276-291.

Beyond the Milky Way. N. Y., *Scribner*. 105 pp.

Heat from the stars. *Scribner's Mag.*, 79, pp. 47-58.

1927

The sun as a research laboratory. Presented at meeting of Franklin Institute, May 18, 1927; *Jour. Franklin Inst.*, 204, pp. 19-28.

The fields of force in the atmosphere of the sun. *Nature*, 119, pp. 708-714.

The Huntington Library and Art Gallery: The new plan of research. *Scribner's Mag.*, 82, pp. 31-43.

1928

Work for the amateur astronomer. II. How to build a small solar observatory. *Pub. A. S. P.*, 40, pp. 285-302.

An inexpensive solar telescope and spectrohelioscope. *Nature*, 121, pp. 676-680.

The Astrophysical Observatory of the California Institute of Technology. *Science*, n. s., 68, pp. 435-437.

Solar research for amateurs. In *Amateur Telescope Making*. 2d ed. N. Y., *Sci. Amer. Pub. Co.*, pp. 180-202 (Pt. IX).

Science and the wealth of nations. *Harper's Mag.*, 156, pp. 243-251.

The possibilities of large telescopes. *Harper's Mag.*, 156, pp. 639-646.

Exploring the solar atmosphere. *Scribner's Mag.*, 84, pp. 46-55.

New light from the nebulae on the nature of matter. *Research narratives*, 3, pp. 113-116.

1929

The spectrohelioscope and its work: I. History, instruments, adjustments, and methods of observation. *Ap. J.*, 70, pp. 265-311; *Mt. W. Contr.*, No. 388.

Spectroheliograph: article in *Encyclopaedia Britannica*, 14th ed., 21, pp. 178-180.

Solar research for amateurs. I and II. *Sci. Amer.*, 140, pp. 302-305, 436-438.

Building the 200-inch telescope. *Harper's Mag.*, 159, pp. 720-732.

1930

The spectrohelioscope and its work: II. The motions of the hydrogen flocculi near sun-spots. *Ap. J.*, 71, pp. 73-101; *Mt. W. Contr.*, No. 393.

Práce pro astroma-amatéra. *Říše hvězd* (Prague), 11, pp. 1-7, 21-27.

1931

The spectrohelioscope and its work: III. Solar eruptions and their apparent terrestrial effects. *Ap. J.*, 73, pp. 379-412; *Mt. W. Contr.*, No. 425.

The spectrohelioscope and its work: IV. Methods of recording observations. *Ap. J.*, 74, pp. 214-222; *Mt. W. Contr.*, No. 434.

Some of Michelson's researches. *Pub. A. S. P.*, 43, pp. 175-185.

A solar observatory for the amateur. *Sci. Amer.*, 145, pp. 244-247.

Signals from the sun. *Scribner's Mag.*, 90, pp. 36-42.

Signals from the stars. *N. Y. Scribner*. 138 pp.

Article written in accepting the Gold Medal of the Holland Society of New York. *Holland Soc., Quarterly circular*, "De Halve Maen", 7, No. 3, pp. 2-4.

1932

Cooperative research with the spectrohelioscope. Third report of Commission appointed to Further the Study of Solar and Terrestrial Relationships, pp. 78-79.

1933

Solar research for amateurs. *In Amateur Telescope Making*, 3d ed. N. Y., *Sci. Amer. Pub. Co.* pp. 180-214 (Pt. IX).

1934

Deeper into space. *Atlantic Mo.*, 153, pp. 463-471.

Horizons. *Vo-Mag.*, 2, p. 16.

1935

Sir Arthur Schuster. *Ap. J.*, 81, pp. 97-106.

The Astrophysical Observatory of the California Institute of Technology. *Ap. J.*, 82, pp. 111-139.

The brightness of prominences as shown by the spectrohelioscope. *M. N.*, 95, pp. 467-468.

Solar magnetism. *Nature*, 136, pp. 703-705.

Address of the President (in absentia), International Council of Scientific Unions, Brussels, 1934. *Reports of Proceedings*, pp. 4-10.

GEORGE ELLERY HALE—ADAMS

Solar research for amateurs. *In* Amateur Telescope Making. 4th ed. N. Y., Sci. Amer. Pub. Co. pp. 180-214 (Pt. IX).

The magnetic periodicity of sun-spots. Pieter Zeeman Jubilee volume, pp. 63-70.

1936

The Astrophysical Observatory of the California Institute of Technology:

The 200-inch reflector. *Nature*, 137, pp. 221-228.

Transportation of the 200-inch mirror. *Nature*, 137, pp. 1017-1018.

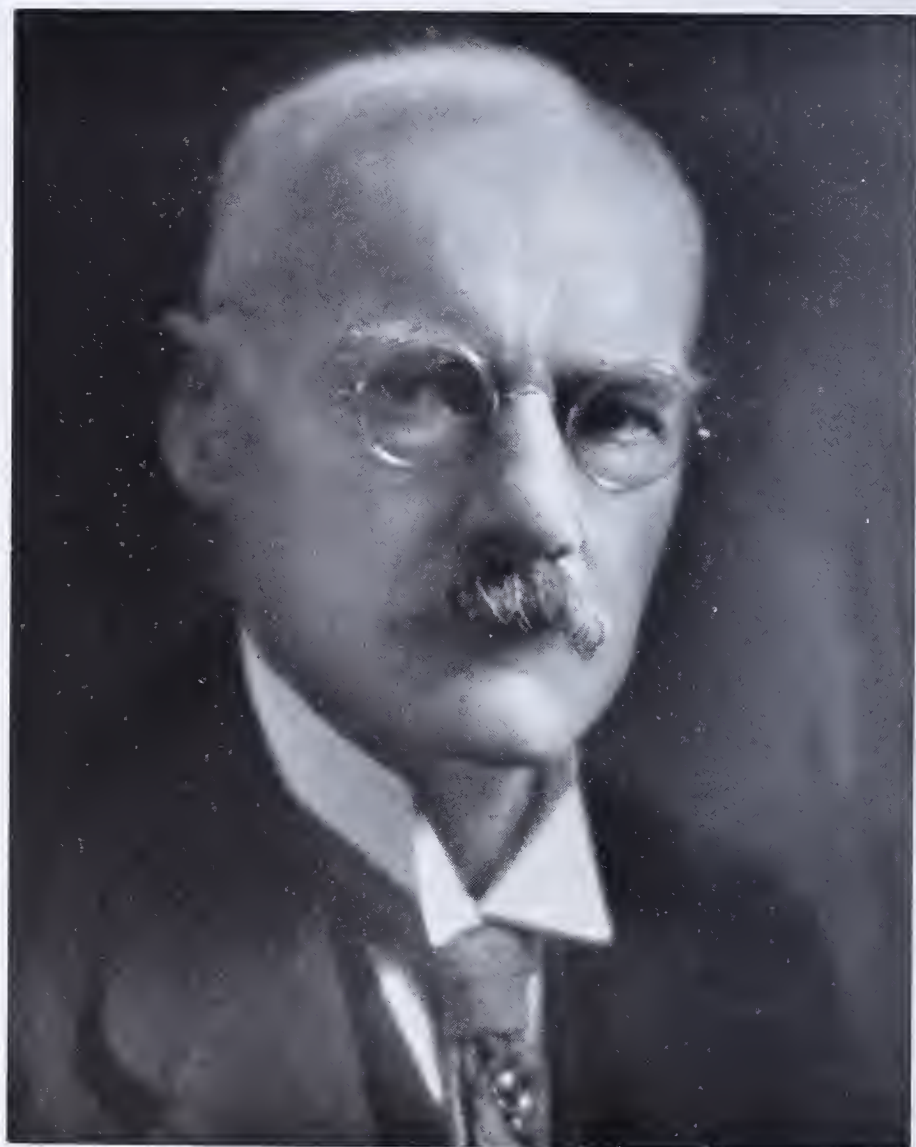
El Observatorio Astrofisico del Instituto de Tecnologia de California. El reflector de 200 pulgadas. *Revista Astronomica* (Buenos Aires), 8, pp. 2-6.

The 200-inch telescope. *Sci. Amer.*, 154, pp. 237-240, 293.

1938

Magnetic observations of sun-spots, 1917-1924. (With S. B. Nicholson.)

Wash., Carnegie Inst., 2 v. Papers of the Mount Wilson Observatory, 5, Pt. 1-2; Carnegie Inst. Wash. Pub., No. 498.



Ernest W. Brown

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OF

ERNEST WILLIAM BROWN

1866-1938

BY

FRANK SCHLESINGER and DIRK BROUWER

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PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1939

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## ERNEST WILLIAM BROWN

1866-1938

BY FRANK SCHLESINGER AND DIRK BROUWER

Ernest William Brown's forbears on both sides lived at Hull, England, or in its immediate neighborhood. His father's father, William Brown (born 1806) was in early life a sailor, later a ship owner and a ship broker. His father (1837-1893), also named William, was for part of his life a farmer and later a lumber merchant. In 1863 he married Emma Martin (1839-1870), by whom he had four children, two boys and two girls. Of these Ernest (1866 November 29 to 1938 July 22) was the second oldest. In 1870 a scarlet fever epidemic carried off his mother and his younger brother. Ernest was not quite four years old at this time and he and his two sisters were looked after by a maiden aunt for about five years, when his father married again.

When Ernest was six years old he began to attend a day school in Hull. The master was at once impressed by his talent for music and urged his father to let the boy prepare for a musical career. This plan seems to have been given serious consideration; and later Ernest, and his elder sister Ella, studied the piano under the guidance of their step-mother. But later his tastes turned toward mathematics in which he greatly excelled both at the day school and at the Hull and East Riding College. Upon graduation from the latter institution he won a scholarship at Christ's College, Cambridge, tenable for three years.

This was in 1884. Then, as now and in all the intervening years, Cambridge was a great center for mathematics and the mathematical sciences; and then, as now, there were gathered together a company of mathematicians who have left a deep impression upon the science of the day. Of this company John Couch Adams (1819-1892) was the one whose work bore closest relationship to Brown's future research. One would have thought that it was he to whom Brown would be most deeply indebted for inspiration and guidance. But as a matter of fact they saw very little of each other, and whatever benefit Brown

may have derived from Adams was chiefly through a perusal of his published work. It was George Howard Darwin (1845-1912), Plumian Professor of Astronomy and Experimental Philosophy, who was to prove his mentor. Darwin early recognized Brown's ability and admitted him into an intimate friendship that was to last till his death in 1912.

Brown was graduated B.A. in 1887 as sixth wrangler. This ranking was a surprise and a disappointment to his friends at Cambridge who were confidently expecting him to gain the senior wranglership. But Brown had no excuses to make and looking back, after the lapse of years, modestly said that his was a case of late development. He remained at Cambridge for three more years as a Fellow of Christ's College, receiving his master's degree in 1891. Then as now Cambridge excelled in mathematical training without paying too much attention to what application the student proposed to make of his mathematics; and it is interesting to know that Brown very narrowly missed going into meteorology, and, on another occasion, into practical astronomy. His definite turn to mathematical astronomy as a career came in 1891 when he was offered a place as instructor in mathematics at Haverford College near Philadelphia, a post that carried with it the directorship of the observatory. He was soon (1893) promoted to a full professorship. Here he remained until 1907 when he was called to Yale University.

While he was still at Cambridge, Darwin had called his attention to the great memoir on the lunar theory by George William Hill (1838-1914), published in 1877, with the advice that the memoir was well worthy of careful study. By the time that Brown came to America he had made up his mind to undertake the formidable task of computing the moon's orbit on this theory. In an address in 1914 he said: "My own theory, which was completed a few years ago, is rather a fulfillment to the utmost of the ideas of others than a new mode of finding the moon's motion. Its object was severely practical—to find in the most accurate way and by the shortest path the complete effect of the law of gravitation applied to the moon. It is a development of Hill's classic memoir of 1877." It does not give the emphasis that it should to his own resourcefulness in

finding the most accurate solution by the shortest path. Hill's theory left plenty of room for the exercise of such resourcefulness before it could be numerically applied. In a series of publications beginning in 1891 Brown presented theoretical developments and their numerical application to the coefficients of certain classes of inequalities in the motion of the moon.

Brown's work on the theory of the moon's motion may be divided into two separate chapters: (1) the solution of the main problem, that is the motion of the moon under the attraction by the earth and the sun only, all three bodies being treated as spherical and the center of mass of the earth-moon system being supposed to move in an elliptic orbit round the sun. (2) The evaluation of the effects upon the moon's motion due to gravitational causes that were ignored in the solution of the main problem. These are mainly the direct and indirect attractions of the planets, and the deviations from mechanical sphericity of the earth and the moon.

There is nothing haphazard about his execution of this task. Each new phase of the work was preceded by careful preparatory studies. Once these had led him to adopt a definite plan, he would begin the systematic work, and let nothing interfere with its progress. This careful planning explains the absence of serious delays by unforeseen difficulties. It also accounts for the long period that preceded the systematic development of the theory which did not begin until 1894, when he completed the plan for the solution of the main problem (8).<sup>\*</sup> At that time he had become so familiar with the entire subject that writing his "Introductory Treatise on the Lunar Theory" (11), still the standard text-book on this subject, hardly retarded the progress of his own theory.

The "Theory of the Motion of the Moon" was published in five parts in the *Memoirs of the Royal Astronomical Society*, 1897-1908, (19). The fourth part, 1905, concluded with a summary of the solution of the "main problem." Both as to completeness and accuracy this solution surpassed the work of Brown's predecessors to a remarkable degree. Few terms having coefficients in longitude and latitude exceeding  $7001$

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<sup>\*</sup> These numbers refer to the bibliography that follows.

were not included, and in the great majority of terms the uncertainty did not exceed  $''001$ . In Hansen's theory some coefficients were in error by some tenths of a second of arc; Delaunay's theory, on account of the slow convergence peculiar to his development, contained a few terms that were in error by as much as a whole second of arc.

The accuracy was confirmed by the preliminary results of the numerical verification of this part of the lunar theory that was carried out by his former pupil, Dr. W. J. Eckert, during the last few years of Brown's life.

The remaining parts of the lunar theory, and, more especially, the planetary perturbations in the moon's motion are among the most difficult subjects in celestial mechanics. Brown's most original work was in this field. One phase of this subject is the theory of the secular accelerations in the moon's motion produced by the secular diminution of the eccentricity of the earth's orbit. For over a century it had been among the most laborious and controversial parts of the lunar theory. Newcomb, in 1895, derived a remarkable theorem that rendered the derivation a relatively simple matter. But Newcomb used Delaunay's developments exclusively; the slow convergence produced an uncertainty that amounted to about five percent of the secular acceleration in the moon's mean longitude, exclusive of uncertainties present due to imperfect knowledge of the planetary masses. Brown succeeded in deriving a new theorem, related to Newcomb's, that enabled him to abbreviate the necessary computations, and to reduce the uncertainty of the result in the case of the mean longitude to only one-third of one percent. In order to accomplish this, judicious use was made of both Delaunay's expressions and the results obtained in his own theory. This derivation (15, 16) was one of Brown's favorite contributions to celestial mechanics.

When Brown began his work on the lunar theory it was known by Newcomb's researches that large unexplained differences existed between Hansen's theory and the moon's observed motion. The question whether these differences could be ascribed to imperfections of the gravitational theory thus became one of the most urgent problems in gravitational astronomy. Its solution required a reliable determination of the planetary

perturbations in the moon's motion. This work was done independently by Radau (1835-1911), Newcomb (1835-1909), and Brown. Of these determinations Brown's was the most complete; moreover, his comparison (46) of the three results left very few discrepancies unexplained.

The direct planetary perturbations were published separately as an essay which obtained the Adams Prize in the University of Cambridge for the year 1907 (44). The fifth and last part of the lunar theory appeared a year later (19).

The successful completion of the theory was merely a milestone in Brown's work on the moon's motion. It was followed immediately by the construction of new lunar tables and a comparison of observations with the new theory. The latter had to be carried out simultaneously with the construction of the tables in order to secure the best possible constants for the final tabulations.

The construction of the tables presented numerous problems totally different from those present in the development of the mathematical theory. Brown's theory, because it was so much more complete than Hansen's, required the tabulation of over three times the number of terms included in Hansen's tables. In order not to triple the work of the ephemeris computer, improved methods of tabulation were required.

With the very efficient aid of Dr. H. B. Hedrick (1865-1936), who was employed as chief computer for nine years, Brown proved himself equal to this task. Hedrick had been connected with the Nautical Almanac Office in Washington for twenty-four years, and possessed exceptional qualities as a computer in the highest sense. In after years Brown grasped every opportunity to emphasize the importance of Hedrick's share in the construction of the lunar tables.

The most ingenious new feature of the tables is the arrangement of the single-entry tables. They occupy less space than in Hansen's case, and are more convenient to use. This is particularly due to their being completely re-entrant; that is, after the value of the argument of each table has been found for a certain instant, the tabular values for succeeding half-day intervals for a whole year can be found without recomputation of the argument or change of the interpolating factor.

The construction of the tables and their publication were financed by the Lunar Tables Fund provided by Yale University; it involved an expenditure of some thirty-four thousand dollars. The three volumes of the "Tables of the Motion of the Moon" (95) were published by the Yale University Press in 1919. They had been printed at the Cambridge University Press. The 660 pages of tables and explanations for their use set a standard of perfection for works of this nature that will not easily be surpassed.

The numerical values for the constants used in the tables had been obtained in a series of contributions (63, 67, 68, 69, 71, 74, 75) from a comparison with the Greenwich observations in the years 1750 to 1900. This mass of some 20,000 observations had been prepared and analyzed by Cowell, a continuation and extension of an undertaking started by Airy (1801-92). The accuracy of the constants is, therefore, primarily due to Cowell's admirable analysis. An interesting confirmation was obtained in 1932 by Spencer Jones who, in a revision of Newcomb's occultation work, made an independent determination of the constants from totally different material, and derived results that are in excellent agreement with Brown's values.

With a few specific exceptions the tables represent the moon's motion according to gravitational theory with the best available constants. These exceptions are: (1) The motions of the perigee and node as used in the tables are the observed values rather than the theoretical ones. The main disagreement was in the motion of the perigee. Shortly before his death Brown showed (190, 192) that this was caused by the omission of certain terms of high order in the theory. These additional terms were then found to bring the theoretical value close to the observed motion. Further clarification of this point, that has been the subject of so many discussions, is expected to be one of the most interesting results of Eckert's numerical verification now in progress at Columbia University. (2) An empirical term with coefficient  $10''.71$  and period 257 years was included in order to eliminate the major part of the fluctuations in the moon's mean longitude during the past three centuries. (3) The oblateness of the earth's figure as used in the tables is  $1/294$ , the value adopted by geodesists being  $1/297$ . (4) The

quantities depending upon the moon's figure are tentatively assumed values that must eventually be improved after a more complete knowledge of the moon's physical librations has been gained. (5) The secular acceleration in the moon's mean longitude as used in the tables is the theoretical value. It is known that this amount should be increased on account of the tidal retardation of the earth's rate of rotation. When the tables were constructed this subject was in such a state of flux that Brown preferred to omit this addition. The theoretical evaluation of tidal friction in shallow seas by Taylor and Jeffreys and also the definitive evaluation by Fotheringham (1874-1936) of the secular accelerations of the moon and the sun from a discussion of ancient eclipses, occultations, and equinox observations all appeared shortly before or shortly after the publication of the lunar tables.

For ordinary purposes of computation of the moon's position these details are of little or no consequence. It is only in refined special investigations that they become important. As a whole, after a lapse of more than twenty years, we conclude that Brown used excellent judgment in deciding upon these matters.

The new tables have been used for the calculation of the moon's place in most national ephemerides since 1923. The great improvement over Hansen, especially in the short-period terms, is immediately apparent from the small range of the residuals from observations in any one year.

That the new theory did not account for the large fluctuations in the moon's mean longitude must have been a great disappointment to Brown, but this negative result stimulated him to further research. His first publication on the subject (52) was in 1910; he continued to deal with it in occasional publications (63, 66, 73, 96, 113) until at last, in 1926, he published his well-known paper "The Evidence for Changes in the Rate of Rotation of the Earth and their Geophysical Consequences . . ." (120). In this publication he accepted the explanation that the fluctuations in the moon's mean longitude including the great empirical term, were no real deviations from its gravitational path, but caused by irregular variations in the earth's rate of rotation.

Before taking this stand he had examined and rejected the theoretical possibility of numerous other hypothetical explana-

tions. Any possible cause, even if it did not appear to hold out much promise, was submitted to a searching test. Some of these hypotheses were his own, some were suggested by other astronomers. Most frequently mentioned were the following: perturbations produced by an assumed equatorial ellipticity of the sun; magnetic attraction between the earth and the moon, and between the sun and these two bodies; a resonance effect produced by the physical librations in the moon's rotation; deviations from the Newtonian law of gravitation; perturbations by intramercurial planets; the shading of gravitation by interposing matter; perturbations by a massive meteor swarm. For a long time he considered with some favor the hypothesis of magnetic attraction between the earth and the moon, and possibly the sun.

Looking backward it may seem surprising that he did not accept the variability of the earth's rotation earlier. Since the explanation requires that other bodies in the solar system exhibit fluctuations similar to those in the moon's mean longitude but with smaller amplitude, the residuals in the longitudes of these other bodies provide the essential test. Newcomb had found some similarity with residuals obtained from transits of Mercury, but not enough to be quite convinced of its reality. In 1914 Brown exhibited the similarity with the curves obtained from the residuals in the longitudes of the sun and of Mercury; Glauert added Venus, and Ross added Mars. Brown discussed the subject again in 1920 (96). While mentioning all this evidence in favor of the explanation, he rejected it because "causes requisite to produce the amount required seem to be absent." This statement throws light upon his method of attack. In dealing with this subject, as well as on many other occasions, he showed his strong preference for emphasizing the mechanical side of a problem, instead of arriving at its solution by a thorough discussion of observational data.

The mechanical explanation of changes in the rate of rotation presents serious difficulties. It requires oscillatory changes in the earth's moment of inertia about its axis of rotation. If the entire mass expands and contracts uniformly the maximum change in the radius required is five inches below or above its mean value; if it is a crustal phenomenon, say to a depth of

80 kilometers, the changes in radius required would be thirty times as great, or about twelve feet below or above its mean value. No physical or chemical causes for such changes are known.

In a final review of the subject (194) Brown stated: "My own idea is to imagine the existence of a layer of material not too far from the surface which is at or near a critical temperature, the latter being defined as one in which a small change of temperature produces a relatively large change of volume. . . ." He then suggested that the hypothesis be tested by a device which could measure very small changes in the opening of a fissure. If a number of them were placed in various parts of the earth, especially in those regions where mountain building is known to be going on, the information needed could be obtained when the next great change in the earth's rate of rotation occurs. The formulation of this *ad hoc* hypothesis is again an illustration of his desire to penetrate to the mechanical cause of the phenomenon.

The immediate reason why Brown took up the subject in 1926 was the appearance, a year earlier, of a paper by Innes (1861-1933) in which he concluded, mainly from a study of transits of Mercury: "When allowance is made for the variability of the rotation of the earth, the moon's motion will probably be found to be purely gravitational. The inclusion of empirical terms confuses." Brown's own presentation contained a very full discussion of the entire subject, but the observational evidence was limited to a qualitative demonstration of the general similarity between the curves obtained from the residuals in the sun's longitude and the total fluctuation in the moon's mean longitude. One of the important results of his publication was that it brought to an end the unfortunate distinction between major and minor fluctuations that had confused the issue for many years. The eagerness with which the astronomical world received his contribution was a striking demonstration of Brown's authority. The fact that he had accepted the variability of the earth's rotation immediately raised the subject from the level of doubtful conjecture to one of full scientific standing. Numerous further discussions by other astronomers appeared in rapid succession. Outstanding among these were the discus-

sions by de Sitter (1872-1934) and by Spencer Jones whose thorough analyses of all relevant observational data essentially confirmed Brown's conclusion.

From 1926 on he gave much attention to comparisons of observations of the moon with the tables. This led to his occultation campaign, supported by many astronomers all over the world.

In the course of his long career he found several opportunities to apply his knowledge of the moon's motion to the study of related problems. After a discussion with Jackson, who had developed a plan to apply Delaunay's theory to the motion of the eighth satellite of Jupiter, Brown took up this problem (102). He overcame the extremely slow convergence by ingenious extrapolations, but was apparently not satisfied with the results. Soon afterwards he began an entirely numerical development of the theory by a totally different method (155, 187).

During the last three years of his life the main problem of the lunar theory again held his almost exclusive attention. In this period he treated the stellar case of the problem of three bodies (188, 189, 190, 191), and followed with the greatest interest Eckert's numerical verification of the solar perturbations in the moon's motion, undertaken at Brown's suggestion. The presentation of the particular form of the equations of motion used in this undertaking is contained in his last publication (195).

Next to the lunar theory he was attracted primarily to the problems of planetary motion. Until 1908, when he finished the lunar theory; his explorations in this field had always been closely connected with the study of the moon's motion. From then on they became independent. A theoretical study of the motion of bodies near the Lagrangian triangular points (54, 55) was later followed by a general theory applicable to all planets of the Trojan Group, and particularly to orbits with large amplitudes of libration (103, 104). Soon after his earlier work on this problem he began a more general study of resonance in planetary motion (58, 61, 105, 106). This problem, related to that of the gaps in the ring of asteroids, had been treated on numerous previous occasions by other mathematical astronomers. Brown's contributions had many original features;

he was probably the first to see clearly that "the calculus of probabilities is more likely to lead to further information than the logical processes of analysis" (137).

The more abstract phases of celestial mechanics could hold his attention for limited times only. Gradually he became more particularly interested in the study of practical methods for planetary theories. In this connection he dealt extensively with the development of the disturbing function (132, 133, 145, 146, 147, 148, 153, 167). This led eventually to the construction with Brouwer of the "Tables for the Development of the Disturbing Function" (172).

He concentrated upon two different forms of planetary theories: the variation of arbitrary constants, and a method in which a modified true orbital longitude is used as independent variable (143). In the former he limited himself mainly to the indication of abbreviated methods and to the computation of the most important terms of higher order due to the presence of small divisors. His true-longitude method is a modification of Laplace's method. It is not an easy matter to judge its value. Brown used it himself in its original form in the theory of the Trojan Group because it offered some advantages in obtaining the intermediate orbit. Later he decided that for the Trojan Group the method of the variation of arbitrary constants is superior. He used it again in the theory of the eighth satellite of Jupiter. In the introduction to the second part of this theory he wrote: "The method has given rise to complications which make the developments of the terms of higher order difficult to follow, requiring great care if errors are to be avoided." A complete application to an ordinary planetary theory has not yet been made.

In the treatise "Planetary Theory" (178), written in collaboration with Dr. C. A. Shook, a coherent presentation is given of most of his contributions to celestial mechanics that are not related to the lunar theory. If one leaves aside the subject of resonance in planetary motion, which has more theoretical than practical aspects, the general impression of all these contributions is one of great unity of point of view. He aimed at the development and improvement of methods for the numerical calculation of general orbits of planets with an

accuracy comparable with that of modern observations. He stressed that for any particular problem a method should be chosen on account of its efficiency, and that no standard method can be made to fit the needs of every special case. His adaptation and critical use of harmonic analysis to problems arising in this field goes well beyond what had been attempted by others before him. He succeeded in demonstrating that a judicious use of numerical methods permitted their successful application to notoriously difficult problems in planetary motion.

Through most of his life he was working on extensive programs that required years of continued effort, but he would occasionally devote himself to the solution of some isolated problems. The great majority of these are of a gravitational type. To this group belong his contributions to the subject of tides and harmonic analysis of tidal observations (23, 60, 77, 114). His most important application in this field was the analysis of records of Shortt Clocks (158, 159) obtained in the laboratory of A. L. Loomis. In this analysis the minute lunar tidal effect upon the pendulum, and consequently upon the rate, was shown to be present. Another striking gravitational study was his critical discussion of the discovery of unknown masses in the solar system from their gravitational effect on other bodies (154, 160), written after the discovery of Pluto in 1930.

He was less successful with studies suggested by astrophysical problems. He attempted to explain the spiral structure of extragalactic nebulae by a gravitational theory (112, 134), but did not succeed in producing more than an interesting analysis of an improbable model. His method of attack required the postulation of a definite hypothetical model. Given such a problem, reduced to a complicated set of equations, Brown could remove its unessential features and penetrate to the heart of the problem with rare ability and certainty. But not all problems presented by nature can be treated in this manner.

In this connection mention should be made of his attempt in 1900 to explain the sun-spot cycle by assuming it to be caused by a tidal disturbance produced primarily by Jupiter and Saturn (24). The treatment of this problem was somewhat arbitrary, and in later years he rather regretted having written the paper.

The work of any scientist can be judged more accurately

if it is placed in relation to that of his contemporaries in the same field. In Brown's case we have to reach back to an older group in order to find men of comparable stature, namely, Newcomb (1835-1909), Hill (1838-1914), and Poincaré (1854-1912). These four men and their work show as striking differences as are likely to be found among the great men of any period. In the following lines some of these outstanding differences are indicated.

To Poincaré, the pure mathematician, it was incidental that celestial mechanics had an important physical application to the motions of the bodies in the solar system. He was attracted by the mathematical difficulties of the three-body problem, and, even if he dealt with a problem presented by a practical case in astronomy, he would proceed at once to free it from any special features, and reduce it to its purely mathematical nucleus. Numerical applications did not appeal to him, although he thoroughly appreciated such work by others.

To Brown celestial mechanics was applied mathematics. His principal aim was the quantitative solution of the equations presented by specific problems in the solar system. He concentrated upon finding practical methods that would be adequate for attaining the numerical standard set by the accuracy of the observations. As an applied mathematician he did not concern himself too much with more theoretical questions such as those of convergence, although he was well aware of the limitations of the developments that he used. The comparison of theory with observations was to him an evil that he avoided whenever possible. Somehow he was never impressed by the value of a thorough discussion of observational data. His lunar tables would certainly have suffered on this account if Cowell had not made his analysis of the observations and if he had not had Hedrick's assistance.

To Newcomb, the astronomer, theories of the motion of planets and planetary tables were but the means toward obtaining a secure basis for the analysis of observations. Great as Newcomb's accomplishments are in the construction of theories and tables for the four inner and two outer planets, his principal contribution was the discussion of thousands of observations.

Upon this he built a structure of fundamental astronomy that, after forty years, still stands intact.

No such definite preference for one phase is noticeable in Hill's work. This creative genius was decidedly interested in the purely mathematical questions of the three-body problem. Yet his researches were, as a rule, guided by the practical needs of the solution of specific problems. Hill preferred to test his methods by numerical applications. In these he showed a love for numerical accuracy and the use of many significant figures that reminds one of some of Gauss' publications. This same theorist undertook the arduous task of constructing the theories and tables for Jupiter and Saturn, the most difficult planets in the solar system. He did not hesitate to make the laborious comparisons with observations. Apparently this was to him a necessary part of the whole, which he performed conscientiously, but without showing any of Newcomb's flair in arriving at his results.

The circumstances under which these men worked were quite different, and have a good deal to do with the nature of their accomplishments. Newcomb, the executive, simplified and standardized his methods in order to be able to have the aid of a staff of computers for the execution of the bulk of the computations, too much for any man to undertake alone. Brown did have the help of computers, one or two at a time, but it was a burden to him to supervise their work or to adjust his plans according to the ability of his helpers. He was happiest when he could have the assistance of men to whom he could leave the greater part of the responsibility and, above all, the details. Hill desired to work alone. Even for the theories of Jupiter and Saturn he made all the computations single-handed, using a computer for checking his own work. Poincaré, of course, had no need for computing aid.

The following extracts are taken from a statement prepared by Brown in connection with the semi-centennial celebration in 1938 of the American Mathematical Society. They refer to his relations with Newcomb and Hill.

"My first acquaintance with American mathematics arose from G. W. Hill's classic paper 'Researches in the Lunar Theory.' Professor G. H. Darwin, who had been my chief adviser

during my year of postgraduate work at Cambridge, had recommended a study of his work and particularly this paper, and he added, 'No one seems to know much about it.' It was therefore not unfitting when, in the summer of 1891, Isaac Sharpless, then President of Haverford College, offered me a position on his staff that I should accept.

"My earliest recollections of mathematicians outside our little Haverford and Bryn Mawr group are connected with two visits that first winter to Baltimore and Washington, a letter of introduction to Craig and Newcomb being given me by Morley at Washington. At Washington I found Simon Newcomb in his office in the Department of the Navy—he was then head of the Nautical Almanac Office and had got well started on his chief life-work—the Theories of the Major Planets. I knocked at the door, heard a gruff 'Come', went in and presented my letter. 'Sit down', said Newcomb and read it. 'What do you want?' was his first remark. At that time a shy youngster, just 25 years old and looking about eighteen, I was completely nonplussed as to what to say. I think I managed to stammer out that I chiefly wanted to make his acquaintance and then bethought me to ask about logarithm tables—the only way we had of performing calculation at that time. It occurred to me then that he was Editor-in-chief of the American Journal of Mathematics which was printing my second paper on the Lunar Theory and by recalling this to him I managed to get down to some real talk. He became quite cordial told me not to hurry off and when I did move, gave me a letter to G. W. Hill with instructions where to find him.

"Hill, quiet soul, was the opposite in manner to Newcomb. He was busy calculating when I found him—I think on the theories of Jupiter and Saturn, and he probably gave me advice on the Lunar Theory, though I have no recollection of anything he said. As a matter of fact I had then formed my own plan of procedure and had started work on it, so that advice, if it were given, was somewhat too late for me to use. Not long after, when giving a paper before the National Academy of Sciences, I met Hill again and experienced his great generosity in mentioning a slight error I had found in one of his papers, although it had nothing to do with the paper being read.

"Hill was difficult to get to know. His chief interest outside mathematical astronomy was botany; on that subject my knowledge was and is almost *nil*. So that we had little in common outside the Lunar Theory when we met, and he was doing scarcely anything on that subject then or in later years. We corresponded occasionally with great satisfaction to me, as he wrote fully on any question which was put up to him, apparently finding it easier to express his ideas on paper than by word of mouth.

“My contacts and correspondence with Newcomb were much more frequent. He really deserved to add the Lunar Theory to his other achievements, but never found time to undertake the chief part of it—the solar perturbations. In fact he had persuaded Hill to undertake the theories of Jupiter and Saturn because he saw that they could not otherwise be completed during his tenure of office, and, as a matter of fact, Hill spent ten years over them with little else to do during that time. But Newcomb’s work on another part of the lunar theory was immensely valuable—the collection and discussion of two centuries of occultations bringing to light the fluctuation which he suggested might be an error in the theory, or might be due to a variation in the rate of rotation of the earth; the latter is now fully substantiated by comparison of the theories and observations of the sun and planets. His attempt to compute the planetary inequalities in the motion of the moon was less successful, partly because of an error, but mainly because he tried to carry it out almost wholly through a method for which it is poorly adaptable.”

Although Brown was not very fond of teaching he fulfilled these duties conscientiously and with distinction. At Yale he was relieved from teaching as far as possible, his average being four hours a week. He did not take the trouble to prepare in detail what he was going to say, and as a result he occasionally found himself at a loss how to proceed from one equation to the next. But he always succeeded in promptly extricating himself from such a situation, and it was highly instructive to his pupils to see how accurately and unfailingly his mind worked under such circumstances.

Like some of his predecessors in Celestial Mechanics, Brown never married. His household was presided over for many years by his maiden sister Mildred, his junior by two years. For most of her adult life she made it her chief, almost her sole, concern to see to his comfort and shield him from cares and disturbances. She succeeded in utterly spoiling him. She died a few years before her brother. Thereafter his only close relatives were his widowed sister, Ella Yorke, and her children. They live in New Zealand and, of course, saw Ernest only on very rare occasions.

Brown knew how to play as well as to work. In his youth he was addicted to rowing and to mountain climbing. He kept up his piano playing and up to within a few years of his death he

was an excellent performer, until in fact palsy made it increasingly difficult for him to strike the keys accurately. But he continued to take great pleasure in music in all its forms. He was for a time the head of the New Haven Oratorio Society. He was fond of chess and played a good game; of late years he gave up this amusement as being too severe a mental tax. He then took to cards, especially bridge, but he did not make a conspicuous success of this game. He was an authority on nonsense verse and could recite without a slip long extracts from Gilbert and Sullivan's operettas, from the Bab Ballads and from Lewis Carroll's verse. In his earlier years he read the English classics, but later he devoted his reading time to the detective story. He was an inveterate traveller and used to attend a great number of meetings, scientific and other. There is no doubt that his chief object in going to so many gatherings was to renew his many friendships among his colleagues.

His daily routine was unusual. He would retire rather early in the evening and as a consequence would awaken usually from three o'clock to five o'clock in the morning. Having fortified himself with a number of cigarettes and a cup of strong coffee from a thermos bottle, he would then set to work in earnest without leaving his bed. At nine o'clock he would get up and have his breakfast. Unless he had something especially exciting on hand, he would not return to mathematical work until the next morning, devoting the intervening time to correspondence, teaching and other similar duties. This program he carried out whenever possible, at home, at the houses of friends he was visiting, and even on board ship.

From his early manhood Brown was affected by bronchial troubles, probably as a result of his rowing activities. Just before his retirement in 1932 he suffered an attack of intestinal ulcers. He refused to take the usual treatment for this complaint, admonishing his physician not to try to prolong his life but simply to make him as comfortable as possible. Strange to say this illness cured itself, but left him in a much weakened condition, and the six years that were left to him were a constant struggle for health. But he went about his work undaunted and undismayed. He died at last of sheer exhaustion on July 22, 1938, in his seventy-second year.

CURRICULUM VITAE AND HONORS

- B.A., Cambridge, 1887.  
 Fellow, Christ's College, 1889-95.  
 M.A., Cambridge, 1891.  
 Instructor in Mathematics, Haverford College, 1891-93.  
 Professor of Mathematics, Haverford College, 1893-1907.  
 Doctor of Science, Cambridge, 1897.  
 Fellow, Royal Society, London, 1898-1938.  
 Member, American Philosophical Society, 1898-1938.  
 Joint Editor, Transactions of American Mathematical Society, 1899-1907.  
 Vice-President, American Mathematical Society, 1905.  
 Professor of Mathematics, Yale University, 1907-21.  
 J. C. Adams Prize, Cambridge, 1907, for essay on the "Inequalities in the motion of the moon due to the direct action of the planets".  
 Gold Medal, Royal Astronomical Society of London, 1907, for researches in lunar theory.  
 Vice-President, American Association for the Advancement of Science, and Chairman Section A, 1910.  
 Joint Editor, Bulletin of American Mathematical Society, 1910-13.  
 de Pontecoulant Medal, French Academy of Sciences, 1910, for advancing knowledge of lunar motion.  
 Honorary Fellow, Christ's College, 1911-38.  
 Associate Editor, Astronomical Journal, 1912-38.  
 Fellow, American Academy of Arts and Sciences, 1912-38.  
 Doctor of Science, Adelaide University, 1914.  
 Royal Medal, Royal Society of London, 1914.  
 President, American Mathematical Society, 1915-16.  
 Vice-President, American Alpine Club, 1920-22.  
 Bruce Medal, Astronomical Society of the Pacific, 1920.  
 Sterling Professor of Mathematics, Yale University, 1921-31.  
 Correspondent, French Academy of Sciences, 1921-38.  
 Citizen of United States, January, 1922.  
 Vice-President, American Astronomical Society, 1923-25.  
 Member, National Academy of Sciences, 1923-38.  
 Corresponding Member, Belgian Academy of Sciences, 1926-38.  
 Josiah Willard Gibbs Lecturer, American Mathematical Society, 1927.  
 President, American Astronomical Society, 1928-31.  
 Josiah Willard Gibbs Professor of Mathematics, 1931-32 (first incumbent).  
 Professor of Mathematics, Emeritus, Yale University, 1932-38.  
 Doctor of Science, Yale University, 1933.  
 President, American Association of Variable Star Observers, 1934-36.  
 Doctor of Science, Columbia University, 1934.  
 LL.D., McGill University, 1936.  
 Watson Medal, National Academy of Sciences, 1937.

KEY TO ABBREVIATIONS

- Amer. Jour. Math.—American Journal of Mathematics.  
 Amer. Jour. Sci.—American Journal of Science.  
 Ann. Math.—Annals of Mathematics.  
 Astron. Jour.—Astronomical Journal.  
 Astron. Nach.—Astronomische Nachrichten.  
 Astrop. Jour.—Astrophysical Journal.  
 Biog. Mem. Nat. Acad. Sci.—Biographical Memoirs, National Academy of Sciences.  
 Bull. Amer. Math. Soc.—Bulletin, American Mathematical Society.  
 Bull. Nat. Res. Coun.—Bulletin, National Research Council.  
 Cambridge Rev.—Cambridge Review, Cambridge, England.  
 Haverford Coll. Bull.—Haverford College Bulletin.  
 Haverford Coll. Stud.—Haverford College Studies.  
 Jour. British Astron. Assn.—Journal, British Astronomical Association.  
 Jour. Franklin Inst.—Journal, Franklin Institute.  
 Jour. R. Astron. Soc. Canada—Journal, Royal Astronomical Society of Canada.  
 Mem. R. Astron. Soc.—Memoirs, Royal Astronomical Society.  
 Mo. Notices, R. Astron. Soc.—Monthly Notices, Royal Astronomical Society.  
 Observ.—Observatory.  
 Phys. Rev.—Physical Review.  
 Pop. Astron.—Popular Astronomy.  
 Pop. Sci. Mo.—Popular Science Monthly.  
 Proc. Amer. Acad. Arts & Sci.—Proceedings, American Academy of Arts and Sciences.  
 Proc. Amer. Assn. Adv. Sci.—Proceedings, American Association for the Advancement of Science.  
 Proc. Amer. Phil. Soc.—Proceedings, American Philosophical Society.  
 Proc. Cambridge Phil. Soc.—Proceedings, Cambridge Philosophical Society.  
 Proc. Fifth Int. Cong. Math.—Proceedings, Fifth International Congress of Mathematicians.  
 Proc. London Math. Soc.—Proceedings, London Mathematical Society.  
 Proc. Nat. Acad. Sci.—Proceedings, National Academy of Sciences.  
 Proc. R. Soc.—Proceedings, Royal Society of London.  
 Proc. Second Pan Amer. Sci. Cong.—Proceedings, Second Pan American Scientific Congress.  
 Pub. Amer. Astron. Soc.—Publications, American Astronomical Society.  
 Pub. Astron. Soc. Pac.—Publications, Astronomical Society of the Pacific.  
 Rev. Astron.—Revista Astronomica.  
 Science, n.s.—Science, new series.  
 Sci. Amer.—Scientific American.  
 Sci. Mo.—Scientific Monthly.

Smithsonian Rept.—Smithsonian Institution, Annual Report.  
 Trans. Amer. Inst. Elec. Eng.—Transactions, American Institution of  
 Electrical Engineers.  
 Trans. Amer. Math. Soc.—Transactions, American Mathematical Society.  
 Trans. Astron. Observ. Yale Univ.—Transactions, Astronomical Observa-  
 tory, Yale University.  
 Trans. Cambridge Phil. Soc.—Transactions, Cambridge Philosophical  
 Society.

# BIBLIOGRAPHY OF ERNEST WILLIAM BROWN

Book reviews and some articles not of scientific character have been  
 omitted.

1. A popular article on the future of the earth's history. *Totteridge  
 Park Magazine* (about 1887).

1891

2. On the Determination of a Certain Class of Inequalities in the Moon's  
 Motion. *Mo. Notices, R. Astron. Soc.*, vol. 52, pp. 71-80.
3. On the Part of the Parallaxic Class of Inequalities in the Moon's Mo-  
 tion, which is a Function of the Ratio of the Mean Motions of the Sun  
 and Moon. *Proc. Cambridge Phil. Soc.*, vol. 7, pp. 220, 221.

1892

4. Notes on the Lunar Theory. *Mo. Notices, R. Astron. Soc.*, vol. 52,  
 pp. 408, 409, 1892; vol. 54, p. 471, 1894; vol. 55, pp. 3-5, 1894.
5. On the Errors Produced in Numerical Calculations by the Use of  
 Decimals. *Haverford Coll. Stud.*, no. 11, pp. 50-55.
6. On the Part of the Parallaxic Inequalities in the Moon's Motion  
 which is a Function of the Mean Motions of the Sun and Moon.  
*Amer. Jour. Math.*, vol. 14, pp. 141-160.

1893

7. The Elliptic Inequalities in the Lunar Theory. *Amer. Jour. Math.*,  
 vol. 15, pp. 244-263, 321-338.

1895

8. Investigations in the Lunar Theory. *Amer. Jour. Math.*, vol. 17, pp.  
 318-358.
9. Note on Hansen's Lunar and Planetary Theories. *Mo. Notices, R.  
 Astron. Soc.*, vol. 56, pp. 52, 53.

1896

10. Co-education in the Colleges and Universities of the United States.  
*Cambridge Rev.*, vol. 17, pp. 330, 331.
11. An Introductory Treatise on the Lunar Theory. *Cambridge Univ.  
 Press*, xvi, 292 pages.

12. Note on Mr. Stone's Paper, "Expressions for the Elliptic Coordinates of a Moving Point to the Seventh Order of Small Quantities." *Mo. Notices, R. Astron. Soc.*, vol. 56, pp. 370, 371.
13. On the Application of the Principal Function to the Solution of Delaunay's Canonical System of Equations. *Proc. London Math. Soc.*, vol. 27, pp. 385-390.
14. On the Application of Jacobi's Dynamical Method to the General Problem of Three Bodies. *Proc. London Math. Soc.*, vol. 28, pp. 130-142.
15. On certain Properties of the Mean Motions and the Secular Accelerations of the principal Arguments used in the Lunar Theory. *Proc. London Math. Soc.*, vol. 28, pp. 143-155.

1897

16. On the Theoretical Values of the Secular Accelerations in the Lunar Theory. *Mo. Notices, R. Astron. Soc.*, vol. 57, pp. 342-349.
17. On the Mean Motions of the Lunar Perigee and Node. *Mo. Notices, R. Astron. Soc.*, vol. 57, pp. 332-341.
18. Note on the Mean Motions of the Lunar Perigee and Node. *Mo. Notices, R. Astron. Soc.*, vol. 57, pp. 566, 567.
19. Theory of the Motion of the Moon; Containing a New Calculation of the Expressions for the Coordinates of the Moon in Terms of the Time. *Mem. R. Astron. Soc.*, vol. 53, pp. 39-116, 163-202; vol. 54, pp. 1-63; vol. 57, pp. 51-145; vol. 59, pp. 1-103; 1897-1908.

1898

20. Report on the Recent Progress toward the Solution of Problems in Hydrodynamics. *Proc. Amer. Assn. Adv. Sci.*, pp. 53-64; *Science*, n. s., vol. 8, pp. 641-651.

1899

21. Note on the Values of the Coefficients of the terms of the Third Order in the new Lunar Theory. *Mo. Notices, R. Astron. Soc.*, vol. 60, pp. 124, 125.

1900

22. On the Solution of a Pair of Simultaneous Linear Differential Equations, which occur in the Lunar Theory. *Trans. Cambridge Phil. Soc.*, vol. 18, pp. 94-106.
23. On Tide Currents in Estuaries and Rivers. *Ann. Math.*, second series, vol. 1, pp. 68-71.
24. A Possible Explanation of the Sun-spot Period. *Mo. Notices, R. Astron. Soc.*, vol. 60, pp. 599-606.

1901

25. Modern Methods of treating Dynamical Problems and in particular the Problems of Three Bodies. (*Précis* of four lectures delivered

before the Ithaca Colloquium of the American Mathematical Society, August 21-24, 1901.) Bull. Amer. Math. Soc., series 2, vol. 8, pp. 103-113.

1902

26. On the Small Divisors in the Lunar Theory. Trans. Amer. Math. Soc., vol. 3, pp. 159-185.

1903

27. The Moons of Mars. Philadelphia Public Ledger, June.
28. On the Formation of the Derivatives of the Lunar Coordinates with respect to the Elements. Trans. Amer. Math. Soc., vol. 4, pp. 234-248.
29. On the Variation of the Arbitrary and Given Constants in Dynamical Equations. Trans. Amer. Math. Soc., vol. 4, pp. 333-350.
30. On the Verification of the Newtonian Law. Mo. Notices, R. Astron. Soc., vol. 63, pp. 396, 397.

1904

31. The Laws of Gravitation. Haverford Coll. Bull., vol. 2, no. 3, pp. 17-22.
32. Moon, The. Encyclopaedia Americana, 1904; also 1936 edition, vol. 19, pp. 423-427, 2 plates.
33. George Gabriel Stokes. Phys. Rev., vol. 18, pp. 58-62; also Smithsonian Rept. 1904, pp. 773-777, 1 plate, 1905.
34. On the Degree of Accuracy of the new Lunar Theory, and on the Final Values of the Mean Motions of the Perigee and Node. Mo. Notices, R. Astron. Soc., vol. 64, pp. 524-534.
35. The Parallax Inequality and the Solar Parallax. Mo. Notices, R. Astron. Soc., vol. 64, pp. 534, 535.
36. On the Smaller Perturbations of the Lunar Arguments. Trans. Amer. Math. Soc., vol. 5, pp. 279-287, 551.
37. On the Completion of the Solution of the Main Problem in the New Lunar Theory. Mo. Notices, R. Astron. Soc., vol. 65, pp. 104-108.

1905

38. The Final Values of the Coefficients in the New Lunar Theory. Mo. Notices, R. Astron. Soc., vol. 65, pp. 276-296.
39. On a General Method for Treating Transmitted Motions and its Application to Indirect Perturbations. Trans. Amer. Math. Soc., vol. 6, pp. 332-343.
40. Sunspots and Weather. Pop. Sci. Mo., vol. 66, pp. 505-514.

1906

41. With the British Association in South Africa. Pop. Sci. Mo., vol. 68, pp. 5-20, 145-160.

1907

42. The Story of the Moon. American Friend, vol. 14, pp. 292-294.
43. Haverford and its Future. Address to the Class of 1907, Haverford

College . . . Commencement Day, June 14, 1907. Haverford Coll. Bull., vol. 6, no. 3, 18 pages.

1908

44. The Inequalities in the Motion of the Moon due to the Direct Action of the Planets. An Essay which obtained the Adams Prize in the University of Cambridge for the year 1907. Cambridge Univ. Press, xii, 92 pages.
45. The Teaching of Mechanics. *Science*, n. s., vol. 27, p. 350.
46. On the Lunar Inequalities due to Planetary Action. *Mo. Notices, R. Astron. Soc.*, vol. 68, pp. 148-170.
47. On the Lunar Inequalities due to the Motion of the Ecliptic and the Figure of the Earth. *Mo. Notices, R. Astron. Soc.*, vol. 68, pp. 450-455.

1909

48. New Plans for Tabulating the Moon's Longitude. Summary of a paper presented at the tenth annual meeting of the American Astronomical and Astrophysical Society, August 19-21, 1909. *Science*, n. s., vol. 30, p. 729.
49. On an Error in the New Lunar Theory. *Mo. Notices, R. Astron. Soc.*, vol. 70, p. 3.
50. On an Addition to the Theoretical Secular Acceleration of the Moon's Mean Motion. *Mo. Notices, R. Astron. Soc.*, vol. 70, pp. 143-148.
51. On the Plans for New Tables of the Moon's Motion. *Mo. Notices, R. Astron. Soc.*, vol. 70, pp. 148-175.

1910

52. On the Effects of Certain Magnetic and Gravitational Forces on the Motion of the Moon. *Amer. Jour. Sci.*, vol. 29, pp. 529-539.
53. Simon Newcomb. *Bull. Amer. Math. Soc.*, vol. 16, pp. 341-355.

1911

54. On a New Family of Periodic Orbits in the Problem of Three Bodies. *Mo. Notices, R. Astron. Soc.*, vol. 71, pp. 438-454.
55. On the Oscillating Orbits about the Triangular Equilibrium Points in the Problem of Three Bodies. *Mo. Notices, R. Astron. Soc.*, vol. 71, pp. 492-502.
56. On the Progress of the New Tables of the Moon's Motion. *Mo. Notices, R. Astron. Soc.*, vol. 71, pp. 639-650.
57. The Transformation of the Moon's Latitude. *Mo. Notices, R. Astron. Soc.*, vol. 71, pp. 651-660.
58. On Planetary Librations. *Astron. Jour.*, vol. 27, pp. 25, 26.
59. The Relations between Jupiter and the Asteroids. Address of the retiring Vice-President of the American Association for the Advancement of Science and Chairman of Section A, delivered December 28, 1910. *Science*, n. s., vol. 33, pp. 79-93.

1912

60. On a Device for Facilitating Harmonic Analysis and Synthesis. *Mo. Notices, R. Astron. Soc.*, vol. 72, pp. 454-463.
61. On Librations in Planetary and Satellite Systems. *Mo. Notices, R. Astron. Soc.*, vol. 72, pp. 609-630.
62. On the Sum of a Certain Triple Series. *Ann. Math.*, second series, vol. 13, pp. 129-136.

1913

63. The Longitude of the Moon from 1750 to 1910. *Mo. Notices, R. Astron. Soc.*, vol. 73, pp. 692-714.
64. Periodicities in the Solar System. *Proc. Fifth Int. Cong. Math.*, vol. 1, pp. 81-92.
65. George Howard Darwin. *Pop. Sci. Mo.*, vol. 82, pp. 309-311.

1914

66. Address on Cosmical Physics. *British Association for the Advancement of Science Report, Section A, Australia*, 11 pages; also *Nature*, vol. 94, pp. 184-190; also *Science*, n. s., vol. 40, pp. 389-401.
67. The Mean Latitudes of the Moon and Sun. *Mo. Notices, R. Astron. Soc.*, vol. 74, pp. 156-167.
68. The Terms in the Moon's Motion depending on the Node. *Mo. Notices, R. Astron. Soc.*, vol. 74, pp. 392-396.
69. The Perigee and Eccentricity of the Moon, 1750 to 1901. *Mo. Notices, R. Astron. Soc.*, vol. 74, pp. 396-424.
70. Correction of Errors in the New Lunar Theory. *Mo. Notices, R. Astron. Soc.*, vol. 74, p. 424.
71. The Determination of the Constants of the Node, the Inclination, the Earth's Ellipticity and the Obliquity of the Ecliptic from the Greenwich Meridian Observations of the Moon, 1847-1901. *Mo. Notices, R. Astron. Soc.*, vol. 74, pp. 552-568.
72. George William Hill. *Nation*, vol. 98, pp. 540, 541.
73. The Problems of the Moon's Motion. *Observ.*, vol. 37, pp. 206-211.

1915

74. A Correction to the Determination of the Constants of the Node, the Inclination, the Earth's Ellipticity, and the Obliquity of the Ecliptic. *Mo. Notices, R. Astron. Soc.*, vol. 75, pp. 506-508.
75. The Elements of the Moon's Orbit. *Mo. Notices, R. Astron. Soc.*, vol. 75, pp. 508-516.
76. G. W. Hill, 1838-1914. *Proc. R. Soc.*, series A, vol. 91, pp. xlii-li; also reprinted *George William Hill 1838-1914. Bull. Amer. Math. Soc.*, vol. 21, pp. 499-511.
77. A Simple and Inexpensive Apparatus for Tidal Analysis. *Amer. Jour. Sci.*, vol. 39, pp. 386-390.
78. *Theorie des Erdmondes.* (Translated by A. v. Brunn.) *Encyklopädie der Mathematischen Wissenschaften*, vol. 6, part 2, no. 14, pp. 667-728.

79. The Committee of One Hundred of the American Association for the Advancement of Science, Report of the Sub-Committee on the Selection and Training of Students for Research. By E. W. Brown (Chairman), R. G. Harrison, G. A. Hulett, W. Lindgren, *Science*, n. s., vol. 42, pp. 368-370.
80. Reading Term at Cambridge. *Yale News*, vol. 38, no. 189, pp. 1, 6.

1916

81. First of a Series of Articles on College Course of Study. *Yale News*, vol. 39, no. 150, pp. 1, 4.
82. Biographical Memoir of George William Hill, 1838-1914. *Biog. Mem. Nat. Acad. Sci.*, vol. 8, pp. 273-309, Portrait.
83. George Howard Darwin (1845-1912). *Proc. Amer. Acad. Arts & Sci.*, vol. 51, no. 14, pp. 863, 864.
84. George William Hill (1838-1914). *Proc. Amer. Acad. Arts & Sci.*, vol. 51, no. 14, pp. 890, 891.
85. Simon Newcomb (1835-1909). *Proc. Amer. Acad. Arts & Sci.*, vol. 51, no. 14, pp. 908, 909.
86. On the Progress of the New Lunar Tables. Abstract: *Pop. Astron.*, vol. 24, p. 588.
87. The Scientific Work of Sir George Darwin. In *Scientific Papers*, by G. H. Darwin, vol. 5, pp. xxxiv-lv, Cambridge Univ. Press.

1917

88. Determination of the Earth's Shape by Simultaneous Astronomical Observations in North and South America. Address delivered before Section 2 of the Second Pan American Scientific Congress, December 31, 1915. *Proc., Second Pan Amer. Sci. Cong., Section 2*, pp. 271-274.
89. The Moon's Place at the Solar Eclipse of 1918, June 8. *Astron. Jour.*, vol. 30, pp. 176-178.
90. The National Research Council. *New Haven Journal Courier*; reprinted, *The Yale Alumni Weekly*, vol. 26, p. 994.
91. The Relations of Mathematics to the Natural Sciences. Presidential Address delivered before the American Mathematical Society, Dec. 28, 1916. *Bull. Amer. Math. Soc.*, vol. 23, pp. 213-230.

1918

92. Mathematics for Freshmen and Sophomores. *Yale Alumni Weekly*, December 13, 1918.
93. The Position of the Moon at the Eclipse of 1918, June 8. *Astron. Jour.*, vol. 31, p. 112.

1919

94. On the Determination of the Secular Acceleration of the Moon's Longitude from Modern Observations. *Proc. R. Soc. series A*, vol. 96, pp. 69, 70.

95. Tables of the Motion of the Moon. (With the assistance of Henry B. Hedrick,) 3 volumes, tables, 34x26 cm, Yale Univ. Press, New Haven.

1920

96. The Problem of the Moon's Motion. Pub. Astron. Soc. Pac., vol. 32, pp. 93-104.

1921

97. The History of Mathematics. Sci. Mo., vol. 12, pp. 385-413, 1921; also as chapter 1 in The Development of the Sciences, edited by L. L. Woodruff, Yale Univ. Press, New Haven, 1923.
98. On the Passage of a Star Through a Nebula. Astrop. Jour., vol. 53, pp. 169-178.

1922

99. The Age of the Earth from the Point of View of Astronomy. Proc. Amer. Phil. Soc., vol. 61, pp. 283-285.
100. Celestial Mechanics. Report of the Committee on Celestial Mechanics of the National Research Council. E. W. Brown (Chairman), G. D. Birkhoff, A. O. Leuschner, H. N. Russell. Bull. Nat. Res. Coun., vol. 4, part 1, no. 19, 22 pages.
101. The Moon's Mean Motion and the New Tables. Astron. Jour., vol. 34, pp. 52-54.

1923

102. On the Application of Delaunay's Lunar Theory to the Eighth Satellite of Jupiter. Astron. Jour., vol. 35, pp. 1-4.
103. The General Orbits of the Asteroids of the Trojan Group. Astron. Jour., vol. 35, pp. 69-80.
104. Theory of the Trojan Group of Asteroids. Trans., Astron. Observ. Yale Univ., vol. 3, part 1, pp. 1-47, 1923; vol. 3, part 3, pp. 81-133, 1925.

1924

105. An Explanation of the Gaps in the Distribution of the Asteroids According to their Periods of Revolution. Proc. Nat. Acad. Sci., vol. 10, pp. 248-253.
106. Resonance in Asteroid Motion and its Treatment. Abstract: Pop. Astron., vol. 32, p. 219.

1925

107. Gravitational Forces in Spiral Nebulae. Astrop. Jour., vol. 61, pp. 97-113.
108. A Celestial Encounter. In History of Connecticut in monographic form, edited by N. G. Osborn, vol. 2, pp. 539-601, 1925; also reprinted in Jour. Franklin Inst., vol. 202, pp. 127-164, 1926.
109. The Eclipse Observations at the Yale Observatory. Pop. Astron., vol. 33, pp. 180-182.

110. Eclipse Investigations not requiring Special Equipment. *Sci. Amer.*, vol. 132, pp. 14, 15.
111. The Eclipse of January 24, 1925. *Science*, n. s., vol. 61, pp. 10-12.
112. The Effect of Varying Mass on a Binary System. *Proc. Nat. Acad. Sci.*, vol. 11, pp. 274-279, 1925. A Correction. *Ibid.*, vol. 12, pp. 1, 2, 1926.
113. On the Stability of the Earth as a Time Keeper. Congress of the International Union of Geodesy and Geophysics, Madrid, 1924, 4 pages.
114. Tidal Oscillations in Halemaumau, the Lava Pit of Kilauea. *Amer. Jour. Sci.*, series 5, vol. 9, pp. 95-112.
115. The International Astronomical Union at Cambridge. *Pop. Astron.*, vol. 33, pp. 569-574.

1926

116. Comparison of the Greenwich and Photographic Positions of the Moon, 1911-1917. *Astron. Jour.*, vol. 36, pp. 153-155.
117. Discussion of Observations of the Moon at and near the Eclipse of 1925, January 24. *Astron. Jour.*, vol. 37, pp. 9-19.
118. Comparison of the Washington and Greenwich Observations of the Moon for 1923, 4, 5 with the New Tables. *Astron. Jour.*, vol. 37, pp. 29-32.
119. Complement to the Tables of the Motion of the Moon, Containing the Remainder Terms for the Century 1800-1900, and errata in the Tables. (With the assistance of H. B. Hedrick.) *Trans. Astron. Observ. Yale Univ.*, vol. 3, part 5, pp. 157-204.
120. The Evidence for Changes in the Rate of Rotation of the Earth and their Geophysical Consequences, with a Summary and Discussion of the Deviations of the Moon and Sun from their Gravitational Orbits. *Trans. Astron. Observ. Yale Univ.*, vol. 3, part 6, pp. 205-235. Summary: *Proc., Nat. Acad. Sci.*, vol. 12, pp. 406-413.
121. Progress on the Orbit of the Eighth Satellite of Jupiter. Abstract: *Pop. Astron.*, vol. 34, p. 550.

1927

122. Request for More Observations of Occultations. *Astron. Jour.*, vol. 37, pp. 99, 100, 1927. With: Notes to Observers with Limited Astronomical Equipment. *Jour. British Astron. Asso.*, vol. 37, pp. 94-96, 1926; also *Pop. Astron.*, vol. 35, pp. 17-20, 1927. Aufforderung zur Vermehrung der Beobachtungen von Sternbedeckungen. Bemerkungen für Beobachter mit beschränkter astronomischer Ausrüstung. *Astron. Nach.*, vol. 229, col. 225-228, 1927.
123. Changes in the Length of the Day. *Nature*, vol. 119, pp. 200-202.
124. The Earth's Rotation. *Observ.*, vol. 50, p. 90.
125. Gravitational Effects in Spiral Nebulae. Abstract: *Pop. Astron.*, vol. 35, p. 484.

126. On Dr. Fotheringham's Paper entitled "Trepidation." *Mo. Notices, R. Astron. Soc.*, vol. 87, pp. 524-527.

1928

127. Developments following from Newton's Work. In *Sir Isaac Newton, 1727-1927. A Bicentenary Evaluation of His Work*, pp. 111-124, Baltimore.
128. Reductions of Occultations observed in 1926. *Astron. Jour.*, vol. 38, pp. 120-123.
129. Reduction of the Washington Meridian Observations of the Moon for the Years, 1923-6. *Astron. Jour.*, vol. 38, pp. 125-129.
130. Discussion of the Moon's Motion 1923-26. *Astron. Jour.*, vol. 38, pp. 129-132.
131. The Residuals from Occultations of the Years 1923-6. *Astron. Jour.*, vol. 38, pp. 133-138.
132. The Fourier Expansions of  $(1 + a^2 - 2ak \cos \psi)^{-a}$ . *Mo. Notices, R. Astron. Soc.*, vol. 88, pp. 459-465.
133. A Remainder Formula and its Use in the Development of the Disturbing Function by Harmonic Analysis. *Mo. Notices, R. Astron. Soc.*, vol. 88, pp. 624-634.
134. Gravitational Motion in a Spiral Nebula. *Observ.*, vol. 51, pp. 277-286.
135. Results of Observations of the Moon in 1926. *Abstract: Pop. Astron.*, vol. 36, p. 229.
136. Occultations: A Report of Progress. *Pop. Astron.*, vol. 36, pp. 282-284.
137. Resonance in the Solar System. (Josiah Willard Gibbs lecture.) *Bull. Amer. Math. Soc.*, vol. 34, pp. 265-289.

1929

138. The Mean Apparent Error of the Moon's Longitude for 1927. (With Dirk Brouwer.) *Astron. Jour.*, vol. 39, p. 32.
139. Corrections to Certain Articles on Occultations. *Astron. Jour.*, vol. 39, p. 96.
140. Compilation and Discussion of 418 Occultations observed in 1927. (With Dirk Brouwer.) *Astron. Jour.*, vol. 39, pp. 97-109.
141. The Change from 7" to 6" for use with the Occultations observed in 1929. *Astron. Jour.*, vol. 39, p. 109; also *Astron. Nach.*, vol. 235, col. 235-238.
142. The Orbit of Jupiter's Eighth Satellite: Completion of the First Stage. *Abstract: Pop. Astron.*, vol. 37, p. 578.
143. The Planetary Theory with the True Longitude as Independent Variable. *Mo. Notices, R. Astron. Soc.*, vol. 90, pp. 3-17.
144. Practical Astronomy for Amateurs. *Sci. Amer.*, vol. 141, pp. 426, 427, 1929; also reprinted *Jour. R. Astron. Soc. Canada*, vol. 24, pp. 177-183, 1930.

1930

145. Applications of an Expansion Theorem to the Development of the Disturbing Function. *Proc. Nat. Acad. Sci.*, vol. 16, pp. 77-83.
146. The Development of the Disturbing Function in Multiples of the Eccentric Anomalies and in Powers of the Eccentricities and Inclination to any Order. *Astron. Jour.*, vol. 40, pp. 19-23.
147. The Expansion of the Constant Term of the Disturbing Function to any Order. *Astron. Jour.*, vol. 40, pp. 35-38.
148. On a General Method for the Development of the Disturbing Function. *Astron. Jour.*, vol. 40, pp. 61-63.
149. Preliminary Value of the Error of the Moon's Mean Longitude for 1929. (With Dirk Brouwer.) *Astron. Jour.*, vol. 40, p. 91.
150. Compilation and Discussion of 746 Occultations observed in 1928. (With Dirk Brouwer.) *Astron. Jour.*, vol. 40, pp. 185-200.
151. De Como el aficionado ayuda a observar y calcular los movimientos de la luna. *Rev. Astron.*, vol. 2, pp. 201-207.
152. The Eclipse of April 25. *Science*, n.s., vol. 71, pp. 314, 315.
153. On an Extension of the Fourier Theorem giving Rapid Methods for calculating the Constant Part and the Coefficient of any Periodic Term in the Disturbing Function. *Proc. Nat. Acad. Sci.*, vol. 16, pp. 150-156.
154. On the Predictions of Trans-Neptunian Planets from the Perturbations of Uranus. *Proc. Nat. Acad. Sci.*, vol. 16, pp. 364-371.
155. Theory of the Eighth Satellite of Jupiter. *Trans. Astron. Observ.*, Yale Univ., vol. 6, part 4.

1931

156. Analysis of Shortt Clock Records made by the Loomis Chronograph. (With Dirk Brouwer.) *Pub. Amer. Astron. Soc.*, vol. 7, p. 4.
157. Compilation and Discussion of 722 Occultations observed in 1929. (With Dirk Brouwer.) *Astron. Jour.*, vol. 41, pp. 53-69.
158. The Lunar Term in Shortt Clocks. (With Dirk Brouwer.) *Observ.*, vol. 54, p. 166.
159. Analysis of Records made on the Loomis Chronograph by Three Shortt Clocks and a Crystal Oscillator. (With Dirk Brouwer.) *Mo. Notices, R. Astron. Soc.*, vol. 91, pp. 575-591.
160. On a Criterion for the Prediction of an Unknown Planet. *Mo. Notices, R. Astron. Soc.*, vol. 92, pp. 80-101.
161. On a Method of Solving Kepler's Equation. *Mo. Notices, R. Astron. Soc.*, vol. 92, p. 104.
162. The Variation of Latitude. (With W. D. Lambert and Frank Schlesinger.) *Bull. Nat. Res. Coun.* no. 78, pp. 245-277; also as chapter 16 in *Physics of the Earth—II*, Washington.
163. The Age of the Earth from Astronomical Data. *Bull. Nat. Res. Coun.* no. 80, pp. 460-466; also as part 5 in *Physics of the Earth—IV*, Washington.

164. When the State was Icebound. *New Haven Journal Courier*, March 9, 1931.
165. Note on Research Problems. *Nature*, vol. 128, p. 916.

1932

166. Compilation and Discussion of 663 Occultations observed in 1930. (With Dirk Brouwer.) *Astron. Jour.*, vol. 41, pp. 185-196.
167. The Development of the Disturbing Function with Large Values of the Ratio of the Distances. *Mo. Notices, R. Astron. Soc.*, vol. 92, pp. 224-227.
168. Elements of the Theory of Resonance illustrated by the Motion of a Pendulum. *The Rice Institute Pamphlet*, vol. 19, pp. 1-60; also re-issued as a pamphlet in Cambridge, England, 60 pages.
169. Observation and Gravitational Theory in the Solar System. *Pub. Astron. Soc. Pac.*, vol. 44, pp. 21-40.
170. George William Hill. *Dictionary of American Biography*, vol. 9, pp. 32, 33.
171. Stability of the Solar System. Abstract of a paper presented at a symposium at the Harvard College Observatory, on the occasion of the dedication of the Astrophotographic Building, March 23, 1932. *Sci. Mo.*, vol. 35, pp. 108, 109.
172. Tables for the Development of the Disturbing Function with Schedules for Harmonic Analysis. (With Dirk Brouwer.) *Trans. Astron. Observ. Yale Univ.*, vol. 6, part 5, 1932; also Cambridge Univ. Press, 85 pages, 1933.
173. Time and its Measurement. *Trans. Amer. Inst. Elec. Eng.*, vol. 51, pp. 523-526.

1933

174. Change from 6" to 5" in the Reduction of the Occultations observed in 1933. (With Dirk Brouwer.) *Astron. Jour.*, vol. 42, p. 125; also *Astron. Nach.*, vol. 247, col. 327, 328.
175. The Motion of the Moon, 1923-1931. *Mo. Notices, R. Astron. Soc.*, vol. 93, pp. 603-619.
176. Occultations of the Year 1924 Collected and Reduced by the Bond Club. (Communicated with Dirk Brouwer.) *Astron. Jour.*, vol. 42, pp. 145-148.
177. Compilation and Discussion of 859 Occultations observed in 1931. *Astron. Jour.*, vol. 42, pp. 181-193.
178. Planetary Theory. (With Clarence A. Shook.) Cambridge Univ. Press, xi, 302 pages, 1 plate.
179. Revision of the Residuals from Occultations of the Years 1923-6 with Additions. (With Dirk Brouwer.) *Astron. Jour.*, vol. 43, pp. 49-55.

1934

180. Compilation and Discussion of 1199 Occultations observed in 1932. (With Dirk Brouwer.) *Astron. Jour.*, vol. 43, pp. 137-152.

1935

181. Gravitation in the Solar System. The Second Arthur Lecture, delivered at the Smithsonian Institution, January 25, 1933. Smithsonian Rept. for 1933, pp. 181-188, 4 plates.
182. Compilation and Discussion of 1194 Occultations observed in 1933. (With Dirk Brouwer.) *Astron. Jour.*, vol. 44, pp. 129-146.
183. Change from 5" to 3" in the Reduction of the Occultations observed in 1936. (With Dirk Brouwer.) *Astron. Jour.*, vol. 44, p. 176; also *Astron. Nach.*, vol. 257, col. 103, 104.

1936

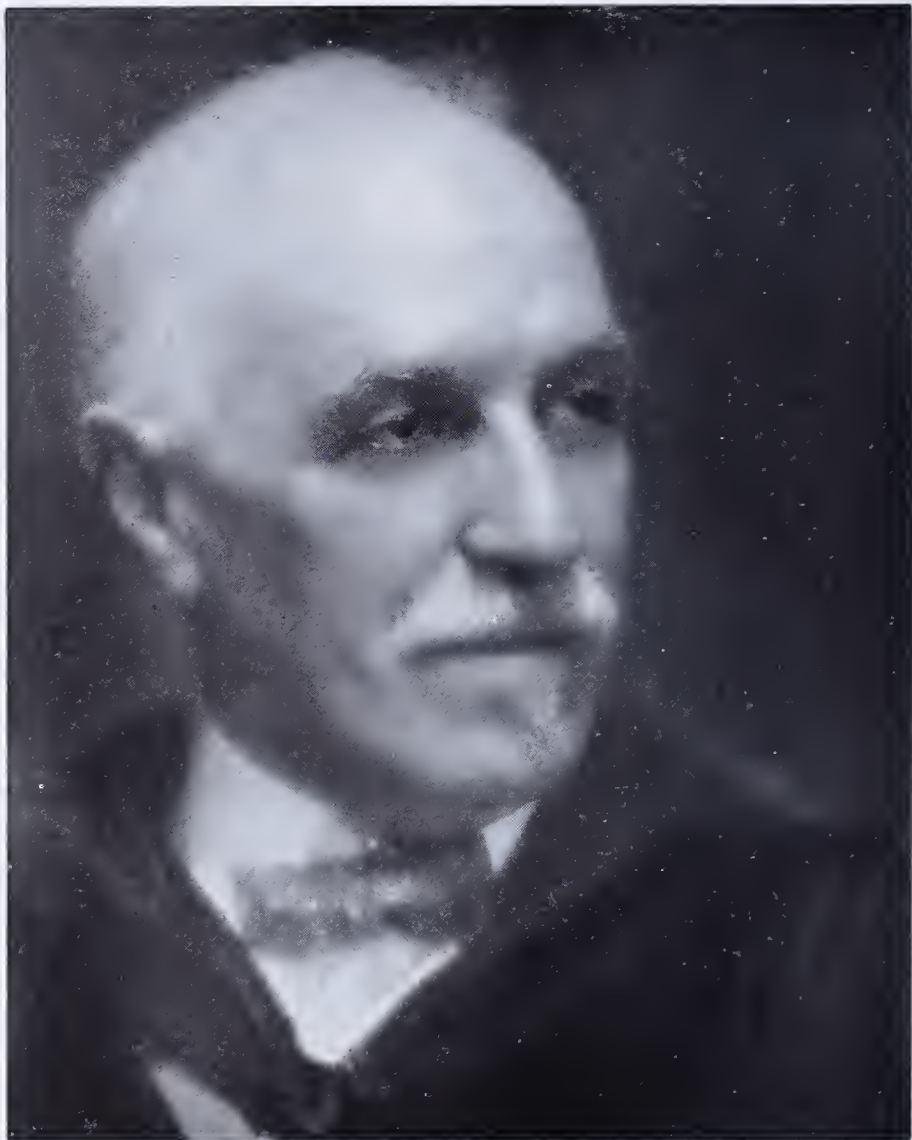
184. On the Calculation of the Principal Parts of the Motions of the Lunar Perigee and Node. *Astron. Jour.*, vol. 45, pp. 84-88.
185. Solutions of the Equations of Variations. *Mo. Notices, R. Astron. Soc.*, vol. 96, pp. 579-587.
186. Compilation and Discussion of 1483 Occultations observed in 1934. (With Dirk Brouwer.) *Astron. Jour.*, vol. 45, pp. 153-160.
187. Theory of the Eighth Satellite of Jupiter, Second Part. (With Dirk Brouwer.) *Trans. Astron. Observ. Yale Univ.*, vol. 6, part 8.
188. The Stellar Problem of Three Bodies. I. Application of Satellite Theory. *Mo. Notices, R. Astron. Soc.*, vol. 97, pp. 56-61.
189. The Stellar Problem of Three Bodies. II. The Equations of Motion with a Simplified Solution. *Mo. Notices, R. Astron. Soc.*, vol. 97, pp. 62-66.
190. The Stellar Problem of Three Bodies. III. The Motions of the Apse and Node with Applications to the Moon. *Mo. Notices, R. Astron. Soc.*, vol. 97, pp. 116-127.

1937

191. The Stellar Problem of Three Bodies. IV. Perturbations in the System  $\xi$  Ursae Majoris. *Mo. Notices, R. Astron. Soc.*, vol. 97, pp. 388-395. Also Addition to Paper IV. *Ibid.*, vol. 97, p. 704.
192. Calculation of the Term in the Motion of the Lunar Perigee with Characteristic  $e^4$ . *Mo. Notices, R. Astron. Soc.*, vol. 98, pp. 170, 171.
193. Compilation and Discussion of 1405 Occultations observed in 1935. (With Dirk Brouwer.) *Astron. Jour.*, vol. 46, pp. 181-188.

1938

194. Changes in the Length of the Day. *Smithsonian Rept. for 1937*, pp. 169-175.
195. The Equations of Motion of the Moon. *Amer. Jour. Math.*, vol. 60, pp. 785-792.



Julius Streicher

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JULIUS STIEGLITZ

1867–1937

BY

WILLIAM ALBERT NOYES

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## JULIUS STIEGLITZ \*

1867-1937

BY WILLIAM ALBERT NOYES

Edward Stieglitz, the father of the subject of this sketch, was born in Gehaus, Thuringia, Germany but spent most of his life in Hoboken, New Jersey, and in New York City. He was an importer of woolen goods and notable for his integrity and high ideals. At one time a letter from the west was directed "To the most honest man in New York City". It was given to Edward Stieglitz. It was in the home of such a man that Julius Stieglitz acquired that sterling honesty which was one of his characteristics to every one who knew him. Neither Edward nor his wife, Hedwig Werner Stieglitz, had extensive academic training. Professor Adolph Werner, the mother's cousin, taught German at the City College, New York. He was a great teacher who loved young people and labored in their interest. There were also Rabbis and other professional men on the mother's side.

Julius and his identical twin, Leopold, were born in Hoboken, N. J., May 26, 1867. Before he was fourteen Julius studied the violincello in New York and later continued the study with Lindner, a noted teacher, at Karlsruhe. His twin brother Leopold played the violin. Julius always retained a great love for music and for the opera.

The twins attended kindergarten in New York City and after that the public schools until they were prepared for the examinations for entrance to the City College of New York. After the boys had passed these, their father interrupted his business activities and took the boys to Europe where they entered the Realgymnasium in Karlsruhe, in 1881, on the basis of their examinations to enter City College.

Before taking the boys abroad the father asked advice regarding professions currently important for young men. He was

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\* The name Julius (Oscar) Stieglitz is given in *Who's Who in America* and in *American Men of Science*, but while the name Oscar must have been given him by his parents, he always disliked it and never used it, and it does not appear in scientific literature. These facts justify its omission from the title of this sketch and also justify the request that it shall not be used in referring to Professor Stieglitz.

told, "engineering and chemistry". Julius was attracted to medicine but when his twin brother, Leopold, chose that profession he preferred to follow a different line and chose chemistry. After preparation at the Realgymnasium he entered the University of Berlin and received the degree of Ph.D. in 1889 at the age of twenty-two.

Summers at Lake George were an important item in his life. The family began to go there in 1874 when Julius was seven years of age. While Julius was in Europe his father purchased an estate, Oaklawn, the larger part of which still remains in the hands of the Stieglitz family. This is a gathering place where they maintain a closely knit family life. Here there was developed a delightful home atmosphere presided over by the gracious, cultured mother, who kept "open house" for artists, authors, musicians and many others who came and went freely. In these surroundings Professor Stieglitz spent many summers after his return from Europe.

These conditions not only compensated the father and mother, in large measure, for the educational facilities they had missed in their youth but also gave to Julius Stieglitz and to his brothers and sisters that broad human culture and sympathy which have enabled them to contribute so much to the progress of our country in many and various directions.

Julius Stieglitz married Anna Stieffel at Lake George, New York, August 27, 1891. She was born in Constance, Baden, Germany, August 28, 1858 and spent her early life in Constance and Karlsruhe. Dr. Stieglitz and Anna Stieffel Stieglitz had three children. Flora Elizabeth was born at Chicago, Illinois, August 10, 1893, but died the following day from injuries received at birth.

Hedwig (Jacobin) was born at Chicago, April 16, 1895. She was educated at the University of Chicago and Rush Medical College. She has served as a health officer in Hammond, Indiana, and is practicing there as an oculist with her husband, Dr. Hugh Alva Kuhn, whom she married March 27, 1920.

Edward (Julius) was born in Chicago, June 6, 1899. After graduating from Rush Medical College he was a National Research Fellow in Medicine. Following this he was an assistant clinical professor in Rush Medical College and was active in

medical research. He also practiced medicine in Chicago. In 1938 he resigned his position in Rush Medical College and gave up his practice to accept a position in Washington as Medical Adviser to the U. S. Department of Labor.

The following extract from the biographical sketch by Dr. McCoy throws a very pretty sidelight on the home life of Dr. and Mrs. Stieglitz.

"One of the strongest traits possessed by Dr. Stieglitz was his love for children. His solicitous care led to such precautions that the family became known in university circles as the 'sterilized Stieglitzes'.

"I shall never forget an incident that illustrates this point. It happened at a departmental picnic on the shore of Lake Michigan. One of the ladies had washed a lot of fine strawberries, brought by Mrs. Stieglitz, in water dipped from the lake and mentioned the fact to Mrs. Stieglitz. At this that good lady threw up her hands and sighed, 'Ach! Du lieber Gott. I had carefully washed them in *distilled* water at home; now the children must get along without strawberries.' To this the children, now both doctors of medicine added, 'and we children caught everything that went the rounds!' "

Anna Stieffel Stieglitz died in Chicago, December 25, 1932.

Professor Stieglitz married Mary M. Rising, then associate professor in chemistry in the University of Chicago, August 30, 1934, in Chicago. She was born July 21, 1889 at Ainsworth, Nebraska.

In June 1932 Dr. Rising had adopted a baby who was born June 1, 1932 and was named Katharine Menardi Rising. After the marriage of Professor Stieglitz and Miss Rising, Professor Stieglitz adopted the child and her name is now Katharine Menardi Stieglitz.

The following true characterization of Professor Stieglitz is copied, with permission from the News Edition of Industrial and Engineering Chemistry for January 20, 1937.

"On January 10 at 7 A.M. Julius Stieglitz died in the calm imperturbability that many will recall as impressively characteristic of him.

"If it be true, as some have maintained, that his deliberation of speech and equanimity of manner were evidences of rigid self-discipline rather than the manifestation of natural disposition, it is but the more remarkable that his life should have been so consistently and his death so magnificently, in character.

"Yet his career of nearly three-score years and ten was one of intense and fruitful activity. The unruffled calm of Julius Stieglitz was as deceptive as that of the confident, skilled, and highly trained athlete, whose sense of timing and coordination and whose superb efficiency of motion enable him to accomplish, without haste and without apparent effort, feats which lesser men vainly pant and strain to equal. His lectures were typical of his mode of action, for their tempo was apparently leisurely: yet such was their economy and precision of statement and their logic of organization that he could present in an hour an exposition for which many lecturers would have found double the time none too much.

"All chemistry was his province. Self-taught in physical chemistry, he applied its principles to the elucidation and improvement of the methods and technics of qualitative analysis, and developed the textbook that became the classic in this branch of instruction.

"In his major field of interest, organic chemistry, he applied the same breadth of knowledge and displayed the same intensity of intellectual curiosity. His theory of indicators, his correlation of chemical structure with color, his investigations of molecular rearrangements and of chemical equilibria, are all permanent landmarks in the progress of the science. To him every problem, every unexplained phenomenon, every apparent anomaly was a personal challenge. He had the profound respect for fact that every scientist must have, but he had moreover the dissatisfaction with bare fact in itself that is the mark of the great scientist. Beyond the fact he sought its antecedents, its consequences and implications, its corollaries.

"This habit of mind left its imprint not only on his own work, but upon his students. Only a clod could have listened to his quiet, unemotional exposition of chemical themes without sensing the enthusiasm and the curiosity that were none the less evident for being undemonstrative.

"Broad as was the range of his chemical knowledge, and numerous as were his chemical achievements, they did not circumscribe his field of interest nor exhaust his capacity for creative work.

"With his twin brother, Leo, who entered medicine as a profession, he had always shared an enthusiasm for that profession. That his medical scholarship extended beyond mere dilettantism is attested by the fact that he was for more than twenty years vice chairman of the Council of Chemistry and Pharmacy for the American Medical Association, that he served as chairman of the National Research Council's Committee on Synthetic Drugs, and that he was for many years prior to his death a con-

sultant for the United States Public Health Service. He both edited and contributed to works designed to acquaint the educated public with the service that chemistry has rendered to medicine.

"With his elder brother, Alfred, the well-known photographic artist, he shared a liking for the camera, and many of his prints might well excite the envy of professionals.

"He found time for both sports and music and he played the cello well enough that his listeners might share his pleasure in it.

"The achievements and accomplishments of Julius Stieglitz and many honors and responsibilities conferred upon him are all matters of record. Of the personality that was Julius Stieglitz it is more difficult to convey a true impression to those who did not know the man intimately. The casual or nonperceptive observer never saw the real Julius Stieglitz.

"It may be that some mistook the air of preoccupation that was his shield against trivialities for professional absent-mindedness. If so, they deceived themselves, for Professor Stieglitz knew all that he cared to know of what went on around him. Indeed, he often knew what was going on in quarters that one would have thought lay beyond the range of his physical perceptions at the moment.

"His public manner often gave the impression of coolness and reserve and it is true that when the occasion demanded he well knew how to wear the mantle of the Herr Geheimrath. To those who sought his aid and advice, however, he invariably displayed the charm and courtesy attributed to southern gentlemen of the old school. He paid those who bespoke his criticism the compliment of speaking frankly. He never saved himself the trouble of framing a constructive suggestion by turning aside a sincere request for comment with a meaningless compliment. Yet his frankest criticisms were tempered with sympathy and were accompanied by sound counsel. His generosity was boundless, and it included his time, thought and effort. We shall not soon see his like again."

The father and mother of Professor Stieglitz were Hebrews but both his first and second wife were Protestants. When he was a young man he read and studied the New Testament carefully and thought it embodied the best philosophy and way of life. One of his friends to whom the first draft of this section was submitted has written that on numerous occasions he said to him "that his greatest joy in life was to help other people".

Lessing in his drama "Nathan der Weise" represents the judge as saying to the three sons who were typical of Judaism,

Christianity and Mohammedanism and who had received identical rings from their father,

“let each one aim

To emulate his brothers in the strife

To prove the virtue of his several ring

By offices of kindness and of love,

And trust in God.”

I am sure that Professor Stieglitz was in close sympathy with the words of Lessing. He had a simple religious faith but often said when discussing some religious question, “I do not know”.

He did not accept the sectarian doctrines of either Jews or Christians. He certainly rejected the belief of some Christians that the death of Jesus was a sacrifice for the sins of the world. In discussing religious questions with his friends he was always careful to avoid saying anything which would hurt their religious convictions.

The words “Jew” and “Gentile” were distasteful to him.

His parents were German but during the war he supported the government as a patriotic American citizen. He once said to me “I can never forgive the Prussians for what they have done to Germany”.

He was a member of the National Research Council and studied hypnotics, novocaine and arsphenamine as war problems. He often visited Edgewood Arsenal for conferences on munitions.

He would have suffered greatly over the fate of Germany. He prophesied part of it when the treaty of Versailles was made. His sympathy was usually with minorities when they needed sympathy. The present state of the Jews would have grieved him greatly. He certainly would not have accepted Hitler's interpretation of Christianity.

When Stieglitz began his work for a Ph.D. dissertation under the direction of Professor Tiemann in Berlin the latter suggested a study of the action of diazobenzene chloride on benzamidoxime,  $C_6H_5C \leq \begin{smallmatrix} NOH \\ NH_2 \end{smallmatrix}$ . As the amidoxime has the properties of an amine, they expected a coupling reaction in which a diazoamino compound would be formed. Instead of this the oxime group was removed from two mols of the amidoxime in the form of

nitrous oxide and the residues formed a cyclic compound with the elimination of ammonia. The same result was obtained when other diazo compounds or an oxidizing agent were used.

The cyclic condensation product and the elimination of ammonia were emphasized in the title of the dissertation. An article was published in *Ber.d.Chem.Ges.* in which the use of the diazo compound was mentioned and the demonstration of the structure of the cyclic compound was developed. The first part of the dissertation contained an exhaustive and scholarly discussion of other condensations of amides with the elimination of ammonia. This is worthy of mention because many of the compounds mentioned in the dissertation were subsequently studied by Professor Stieglitz and his students at the University of Chicago.

After receiving his degree at Berlin, Stieglitz worked for a short time with Victor Meyer at Göttingen. Returning to America he spent a few months as a scholar at Clark University, having been attracted by the brilliant work of John Ulric Nef. In 1890-92 he spent two years in the laboratories of Parke, Davis and Co., in Detroit. His work was chiefly in toxicological analysis. Since the results of his work might lead to the indictment and conviction of a murderer, he felt the responsibility of his tasks very keenly and the occupation became very distasteful to him. He resigned in 1892 with the intention of entering academic work.

In 1892 he was offered a position in the College of the City of New York but, in spite of the fact that he had been married in 1891, he chose to go to the University of Chicago with a very small income.

At the University of Chicago he was advanced as follows:

- 1892-93—Docent.
- 1893-94—Assistant
- 1894-97—Instructor
- 1897-1902—Assistant Professor
- 1902-05—Associate Professor
- 1905-33—Professor
- 1912-24—Director of University Laboratories
- 1915-33—Chairman of the Chemistry Department
- 1933-37—Professor Emeritus

Professor Stieglitz received the honorary degree of Sc.D. from Clark University in 1909 and the degree of Chem.D. from the University of Pittsburgh in 1916.

Professor Stieglitz was awarded the Willard Gibbs Medal in 1923.

He was Hitchcock lecturer at the University of California in 1909; Dohme lecturer at Johns Hopkins University in 1924; Fenton lecturer at the University of Buffalo in 1933; lecturer at the Centenary of the Franklin Institute in 1925.

He was one of the Associate Editors of the *Journal of the American Chemical Society* 1912-19.

He was a member of the National Academy of Sciences; of the American Philosophical Society; of the American Academy of Arts and Sciences; of the American Association for the Advancement of Science, Vice President in 1917; of the American Chemical Society, President in 1917; of the Washington Academy of Sciences; President of Sigma Xi, 1917-19; a member of Soc. Chimique de France.

Member of the Board of Editors of the *Scientific Monographs of the American Chemical Society* 1919-36; member of the international commission for the *Annual Tables of Constants*; President of the Chicago Institute of Medicine 1918; vice-chairman of the council on chemistry and pharmacy of the American Medical Association 1902-24, member of the chemical division of the National Research Council 1917-19; chairman of committee on synthetic drugs 1917-19; vice chairman of the division of chemistry 1919-21; special expert of the U. S. Public Health Service 1918-36.

In the *Journal of the American Chemical Society*, 60, Proc. 3 (1938) Herbert N. McCoy has published a very excellent biography of Professor Stieglitz. With his permission and that of the editor of the *Journal*, I copy the following extracts about which he has intimate personal knowledge that I do not have.

"The record of achievement in research marks Stieglitz as one of the strong men of his time. In addition to splendid teaching and high class research, he carried on other activities of great importance both to his university and to the nation.

"It was no part of Stieglitz' philosophy that teaching, research, and the writing of textbooks should constitute all of one's duties as professor. His horizon had a much wider scope and not many

years had passed before he took an active part in faculty matters. His thorough investigation of the questions involved, his logical and forceful presentation of their significance, and his fairness to both sides soon were recognized. He soon became a key man whose advice on university affairs was eagerly sought; his was the guiding influence in many committees. I will mention only two instances of the many where his services were of great value; and, in later years, there was scarcely an important question about which he was not consulted.

"Of his many memorable achievements, one was that concerned with the conflict between science and the classics. In the early nineties Chicago in common with most other colleges still adhered to a rigid requirement of the classics as a prerequisite for the bachelor's degree even for science students. This inheritance from medieval times was becoming very irksome to the students in physical and biological sciences. The preceding twenty years had witnessed amazing and unprecedented developments in natural philosophy which had now become a group of experimental sciences with seemingly unlimited possibilities. To be required to thumb through musty tomes of Latin and Greek when so much of greater interest and practical value was at hand seemed, to science students, a sacrilegious waste of time. The battle for the removal of this restriction waxed long and hard; Stieglitz was the leader for the cohorts of science.

"In the department of chemistry at Chicago, Stieglitz' students commemorated the ultimate triumph of the science group by presenting him with a parchment scroll expressing their gratitude for his untiring efforts in their behalf; the names of many who have since risen to prominence are appended.

"For one without special training in the subject, Stieglitz was outstanding among his colleagues for having a remarkable knowledge of medicine. This interest arose in early life and doubtless would have led him into that profession but for reasons already mentioned. Perhaps, also, the fact that Mrs. Stieglitz was afflicted with asthma for long years, and that their first child had died at birth and the other two had passed through numerous contagious ailments had much to do with the strengthening of this interest.

"It seems natural then that in 1901 when President Harper in his desire for a medical school at Chicago began to strive for a union with Rush Medical College as a means to this end, Stieglitz should find a place on the committee of which the late Dr. H. H. Donaldson was chairman.

"With only \$50,000 in sight, Harper proposed to begin in a modest way by transferring to the university campus the work of the two preclinical years. After a memorable conference of

President Harper with the combined committees of the two institutions, in which ways and means were thoroughly discussed, Stieglitz asked Dr. Ingals, treasurer of Rush, the critical question he long had had in mind; that was, whether the tuition fees of the transferred students would revert to the university. When Ingals replied in the affirmative, Stieglitz saw at once that with this additional income the union so greatly desired by the University could be safely made with the \$50,000 available. Harper was equally quick to grasp the situation and, in his characteristic fashion, changed the subject immediately and quickly brought the conference to a close. On the way out he said privately to Stieglitz, 'Mr. Stieglitz, let me have your budget by next Saturday morning.'

"Stieglitz had already worked out the details, but to avoid any serious blunders he and Dr. Donaldson made a hurried visit to four leading eastern medical schools, prospectuses of which they had studied. They returned convinced that with the laboratories and staffs at the university, together with the modest sum the president had mentioned as being in sight, they could offer facilities equal or superior to those of the schools they had visited.

"The union of Rush and the university was soon consummated and proved successful from the start. The hitherto nearly empty Hull laboratories were filled with medical students. The departments of anatomy, physiology, pathology, physiological chemistry, and bacteriology became realities instead of mere names, and suddenly took on new life.

"The union with Rush was looked upon by Stieglitz as a vital transfusion of blood into all science departments. He was justly proud of the part he had played in helping to lay the foundation of a great medical institution now a part of the university.

"After the premedical courses had been brought to the university the deanship of the new division was offered to Dr. Stieglitz by President Harper. The former with characteristic farsightedness begged to decline the honor and pointed out to the president that he considered it a better policy to put a Rush medical professor in the deanship.

"Stieglitz' close association with physicians and surgeons during the period of negotiations with Rush Medical College led to lifelong friendships with many leading medical men. This was the beginning of his affiliation with the American Medical Association that lasted officially two decades but actually existed throughout his lifetime. In 1905 he became vice-chairman of the powerful Council of Pharmacy and Chemistry, a responsibility he held until 1924. A record for the year 1920 showed Stieglitz' name on five of the fourteen committees of the Council.

"Dr. Leech, present secretary of the Council, commenting on Stieglitz' services to the American Medical Society says: 'It is proof of his fine judgment and parliamentary ability to note that the rules are essentially the same today as thirty-two years ago. He took great pains in seeing that every safeguard was put in the rules for impartiality of decisions.

"'In his capacity as vice-chairman of the Council Stieglitz exerted a powerful influence on its action during the nineteen years of his tenure of office.'

"The summer of 1914 marked the beginning of the most critical period for chemistry in America. The British blockade that quickly followed the beginning of the World War shut off importation from Germany and thus soon produced a dearth of the previously imported fine chemicals so necessary for medicine and industry. The situation threatened speedy disaster. In the decade that followed Stieglitz played a conspicuous part among the army of loyal chemists whose efforts proved successful in this emergency.

"On January 1, 1917, Dr. Stieglitz became president of the American Chemical Society. The war was now in the middle of its third year. Three and a half months later America joined the cause of the Allies. As never before, war had developed into a conflict of chemists. Few officers of the society faced graver tasks than did Stieglitz and none deserves greater credit than he for his unselfish services in aiding in the stupendous developments that finally brought relief to medicine and industry and chemical independence to America. His notable success was due not only to his sound and extensive knowledge of organic chemistry, to his love of the science and art of medicine, to his deep interest in synthetic drugs, as evinced by his long service on the Council of Pharmacy and Chemistry of the American Medical Association; but, in no small measure, to his subsequent election or appointment to leading positions in organizations where his opportunities for acquiring information about existing conditions were unexcelled.

"In February, 1917, he was appointed chairman of the Committee on Synthetic Drugs of the National Research Council which Committee was the scientific adviser of the Government, and which had been organized the foregoing year under the authority of the National Academy of Sciences. He also held a semi-official position as adviser to the Federal Trade Commission.

"In 1918, he became president of the Chicago Institute of Medicine, an organization having for its object the promotion of medicine through scientific research. At the same time he accepted an appointment in the United States Health Service as Special Expert in Arsenicals.

"The Chemical Foundation was incorporated in February, 1918; the following month Francis Garvan became its president and Dr. Stieglitz its Chemical Adviser. In 1920 he was chosen Consultant for Chemical Warfare Service by General Amos A. Fries. During this period he also maintained his position as Director of the Department of Chemistry at the University of Chicago.

"Thus his facilities for acquiring accurate information regarding the chemical situation in America were unexcelled, and his prestige assured him a sympathetic hearing among those in authority whenever his advice was offered.

"For months the lamentably insufficient supply of synthetic drugs had been an acute problem. Such drugs were formerly imported from Germany almost exclusively. Their number had been much greater than necessary. Many of them had been introduced merely for commercial reasons. Through his various medical connections Stieglitz was able to ascertain that most of these could be dispensed with if but a very small number of the most reliable ones were made available. Only four or five were indispensable and for several reasons it seemed desirable to give these substances new, American names. Arsphenamine was the name coined by Stieglitz for the drug called Salvarsan by the Germans. It is the '606' of Ehrlich and is the recognized cure of syphilis, a disease with which ten million Americans were said to be afflicted.

"Barbital (formerly called Veronal) was the most widely used and reliable hypnotic. Procaine (formerly called Novacaine) was the most useful of all local anaesthetics. Lack of it necessitated the so-called 'Bulgarian operations', those made without anaesthesia. Cincophen (Atophan) was the most efficient drug in the treatment of gout and rheumatism. Phenobarbital (luminal) a sedative and hypnotic, was a specific drug for the prevention of the seizures of epilepsy. Physicians and hospitals were literally begging for this drug for two years before it was made in America.

"As a member of the Committee on Synthetic Drugs, it was the duty of Stieglitz to see that the public received supplies of these indispensable drugs as quickly as possible. To this end he either induced or encouraged certain reliable manufacturers to take up their production. He gave them chemical advice and aided them in obtaining the supplies of auxiliary chemicals that were required, as, for example hydrosulfite, needed to make arsphenamine and bromine, needed in the synthesis of procaine.

"As Adviser to the Federal Trade Commission, his trying duty was to decide to which manufacturers licenses were to be issued, for the production of each drug.

"When, in 1918, it became known to the Federal Health Service that some domestic supplies of arsphenamine were causing serious trouble in the treatment of army cases, Stieglitz, as Special Expert in arsenicals, rendered much valuable service.

"Speaking of the Public Health Service, Dr. George W. McCoy, Chief of the Hygienic Laboratory, says, 'We were urgently in need of the very best advice available anywhere with regard to the standardization as to quality and safety of the preparation of the arsphenamine group. Professor Stieglitz gave unstintingly of his time and effort. It was always a source of great comfort to know that we were proceeding in accordance with the advice and suggestions of Professor Stieglitz.'

"In his capacity as Professor of Chemistry of the University, Stieglitz and his students conducted important researches on arsphenamine in the course of which it appeared to his satisfaction that the specifications given in the patent were not sufficient to guide a trained chemist to the preparation of a safe product. The additional steps which he employed were highly necessary. His investigations on phenobarbital, so long in demand by physicians, led to work by Stieglitz and Mary M. Rising that resulted in the finding of improved methods for its preparation through substitution of methyl esters, which are solids easily purified by recrystallization, for the liquid ethyl esters used originally.

"It was Stieglitz more than any other chemist who stressed the close genetic relationship between the three great classes of organic substances, dyes, war chemicals and medicinals. To these, of course, may be added photographic chemicals. In all his efforts to keep this connection to the forefront he clearly saw that an ultimate solution could only be reached through the establishment in this country of a great dye industry. The dye 'intermediates' are just those chemicals which form the starting material for explosives, war gases, synthetic drugs and photographic chemicals. For economic reasons all must go together. The works and equipment for the manufacture of one class of products serves with little modification for all. It was through his connection with the Chemical Foundation that his greatest efforts on this subject were made. In the record of the trial of this organization, Stieglitz' achievements are recorded in much detail.

"It will be remembered that this was the celebrated case in which the Government tried to set aside as illegal the purchase of over four thousand German patents covering dyes and related substances and their subsequent license to American firms. The Chemical Foundation had acquired title to the patents through their purchase for \$250,000 from the Alien Property Custodian.

"The trial occurred in Wilmington, Delaware, in December, 1923, before the Honorable Hugh M. Morris, United States District Judge. In the course of the trial Stieglitz gave valuable testimony in support of the contention of the defendant (C. F.) that a completed dye industry is the logical and only practical defense against a recurrence of the perilous situation in which the country was placed at the outbreak of the World War.

"He showed clearly to the satisfaction of the court how and why an adequate supply of dye intermediates would provide those chemicals indispensable not only for the manufacture of the dyes themselves but also of explosives, war gases, medicinals, photographic and other chemicals. The climax of Stieglitz' testimony, at least for the non-technical audience, was reached when by permission of the court he illustrated his point by an actual demonstration in which he showed that a given intermediate might serve for the preparation of a dye, a medicinal, or a war gas. He placed a few drops of aniline in each of three test-tubes: in the first he produced the dye, mauve, by oxidation of the crude aniline with a drop or two of ferric chloride. In the second tube he converted the aniline into a medicinal, the popular sedative, acetanilide by the use of acetyl chloride. To the aniline in the third tube he added alcoholic potash and a drop of chloroform. The nauseating phenyl isocyanide formed was convincing as a war gas.

"Referring to Stieglitz' testimony in this suit, Dr. Charles L. Parsons writes: 'I remember how complacently he confounded the opposition who thought they were going to make a point by asking him as to the fees he drew from the Chemical Foundation by quietly stating that he never went into court except in the public interest and consequently never accepted any fee therefor. The statement was very effective in the outcome of the trial, which was won by the Chemical Foundation.'

"Dr. Stieglitz was editor and contributor to the well known book, 'Chemistry and Medicine,' a most interesting and informing document. This contribution of forty-three well known scientists was sponsored by the Chemical Foundation and widely distributed, gratis, by Mr. Garvan as a memorial to his daughter, Patricia, who died in childhood.

"His services as Adviser to the Chemical Foundation are thus described in a letter by Mr. William Buffum, treasurer and general manager of the organization, 'His counsel was often sought and freely given. He was always very cooperative in assisting us to solve our general and special problems. It can undoubtedly be said that Dr. Stieglitz was the foremost exponent of American Chemotherapeutic research. He was one of Mr. Garvan's and my closest friends.' As concrete evidence

of his friendship and admiration, Mr. Garvan created the Stieglitz Foundation which was put at the disposal of Dr. Stieglitz for research on problems in the field of chemistry as applied to medicine.

"In accordance with university regulations Stieglitz' retirement took place in September, 1933, at the age of sixty-five. This change in official status did not prevent his retention of his rooms at Jones Chemical Laboratory, the splendid new home of the department given by George Herbert Jones, where as emeritus, he continued to give advice on departmental matters whenever requested to do so. His students were so loath to give him up that they raised a special fund which permitted him to continue his lectures and direct research.

"Although he had numerous attractive opportunities he had steadfastly refused to act as a consulting chemist for industry during his professorship. It was only after his retirement that he made his first and only connection of this kind, a 'half-time' position on the staff of the Universal Oil Products Company. Here he was associated with the able organic chemists, Egloff, Ipatieff, and others. . . .

"He was a trustee for International Critical Tables as representative of the American Chemical Society and contributed materially to its ultimate successful publication. . . .

"A few years prior to her marriage, Dr. Rising had adopted a baby daughter, Kate. It is not strange that after the marriage, little Kate was soon so firmly established in the affection of Dr. Stieglitz that he adopted her. Professor Stieglitz often spoke feelingly of his fondness for children. A typical remark was, 'Babies, I love them. If I could, I would like to have them in the house all the time!' With his three grandchildren, two sons of Mrs. Kuhn and the daughter of Edward, his desires seem to have been realized. The home thus newly established provided for him those things which for him were so necessary for life and productive work.

"In spite of a most strenuous and time-consuming schedule, Stieglitz found some time for diversions. In his younger days, tennis and billiards were his favorites. Later their place was substituted by golf. Stieglitz never played bridge; once when the subject was brought up, Stieglitz remarked, 'I hate cards, that is, except poker: That's not cards but a play of human nature.'

"A hobby in which he attained results of high artistic merit was photography. This was the field in which his older brother, Alfred, had reached world-wide distinction. Much time was spent at this diversion and many splendid prints attest his skill.

"Up to the time of his last illness, Stieglitz had enjoyed consistent good health.

"To the countless thousands who knew him only by his published works Julius Stieglitz was a chemist of high attainments. To the smaller favored number whose acquaintance was more intimate, his friends, colleagues and students, he was a unique and highly gifted personality.

"His life work formed an exceptionally consistent whole in full accord with his oft-expressed formula of a complete academic career. His activities were apportioned with rare idealism between teaching, research, university administration, and public service. To have made a success in one or two of these lines of endeavor would have assured him a lasting remembrance. His preeminence in all four is a symbol of his versatility and stamps him as one of the outstanding men of his time.

"Although Dr. Stieglitz' solicitous care for the welfare of his family was at all times uppermost in his mind, his personal interests were always subordinated in favor of the attainment of his ideal. As an example of this it is related on excellent authority that he once refused a position as chemist in a major industry at \$50,000 a year.

"It was a rare trait of Stieglitz' character that in the accomplishment of a desired end he did not merely lend his support to the movement. It was his custom to work out in complete detail a plan by which the project could be carried out. Thanks to his prodigious and accurate memory he could take a conflicting mass of details, sift them, discard the irrelevant, and weave the sound arguments into a convincing whole. His opponents were wont to give way with the remark, 'What's the use, Stieglitz is too logical.'

"Perhaps it is in his relations with his students that his memory is most highly cherished. His popularity as a teacher is attested by the significant fact that in all the University, the number of students who made their doctorate with him was greater than with any other professor. Among this number, 118, are many men and women who have achieved a national reputation. Among those whose life's work is complete and who were most widely known are Dr. Otto Folin, Professor of Biological Chemistry, Harvard Medical School, and Dr. Edwin E. Slosson, author of 'Creative Chemistry' and founder of Science News Service. Many others hold high places in the field of pure and applied chemistry.

"The problems he set for his research students were always well thought out in advance, never too difficult or impossible. In the laboratory his attitude was dignified and formal, but quiet and kindly, never seemingly hurried, but never wasting time or words, never neglecting students but always keeping before them the highest ideals. A typical remark, 'I think the compound can exist, Mr. X. In a week you should have at

least ten grams of it,' was taken by the student as an order that he exerted himself to the utmost to fulfill, although its accomplishment won only the equally mild, but greatly valued, approval, 'Very good, Mr. X.'

"Excepting during the earliest years at Chicago, Dr. Stieglitz rarely carried out his researches with his own hands. Nevertheless he was always critical of apparatus and technique. Often when he was forced to disapprove, his criticism was couched in so humorous a vein as only to spur the man to greater effort to improvement. A beginner at research had set up an apparatus that differed greatly from that described in the reference given. On seeing it Dr. Stieglitz remarked, 'That is not the apparatus used by . . . , Mr. X.' When the student excused the fault as due to his faulty knowledge of German, Dr. Stieglitz left without a word; a minute later he returned, placed on the desk the volume in question, opened to the page showing a figure of the apparatus, 'The picture is not in German, Mr. X.'

"On another occasion when a student in qualitative analysis explained that he was evaporating a solution of ammonia to concentrate it Stieglitz' only comment was, 'Go right ahead, Mr. X.'

"To his students Dr. Stieglitz always lent his helpful sympathy when other resources failed. He was ever ready to listen to their problems and was most happy when he was able to work out a satisfactory solution for their difficulties. With students, as with others he had a great reputation for fair dealing. In many cases the advice was more personal, extending as his daughter (Dr. Hedwig Stieglitz Kuhn, M. D.) humorously avers, 'To every subject from the proper selection of the bride's silver to the choice of an obstetrician.'

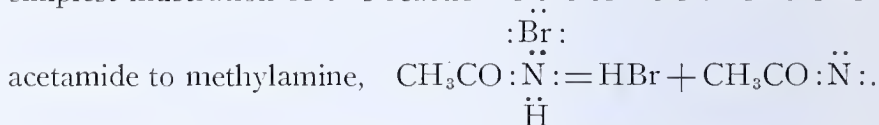
"Always overworked, with his time sorely overcrowded, it is not strange that those who came to his room unannounced to speak on trivial matters often took a brusque dismissal for surliness. This was only apparent and not intended. Until late in life he had no secretary who could protect him from thoughtless intrusions.

"On his part Dr. Stieglitz was always considerate of the time of others. For years the writer's laboratory was directly across the hall from his. He very rarely came informally to discuss matters. Instead it was his custom to place handwritten notes in my box.

"The esteem in which Dr. Stieglitz was held by his contemporaries is amply attested by the many positions of honor and responsibility to which he was called. Little can now be added to his fame by the laudation of his biographer. To employ a favorite maxim of Dr. Stieglitz: 'Let the work show its worth by itself.' The work of Julius Stieglitz is his memorial; a

fitting testimonial to a splendid career. In his work he realized his highest ideal: a life devoted to the advancement of science and the promotion of human welfare."

The first work of importance done by Stieglitz at Chicago was a study of nitrogen halogen compounds published in three papers with Felix Lengfeld. They had hoped that by treating the compound with sodium methylate they could replace the halogen with the methoxy group and obtain a derivative of hydroxylamine. It could not have been understood then that when a halogen atom separates from a nitrogen atom it tends to do so in the positive form, (e.g.: Br, leaving the pair of electrons of the covalence with the nitrogen). For this reason the halogen atom could not be replaced by the negative methoxy group  $\text{CH}_3\text{:O:}$ . Instead of this there is a rearrangement which Stieglitz and Lengfeld recognized in their paper as that of Hoffmann and of Hoogewerf and van Dorp by means of which an amide group,  $\text{CONH}_2$ , is replaced by an amino group. The simplest illustration of this reaction is the conversion of bromo-

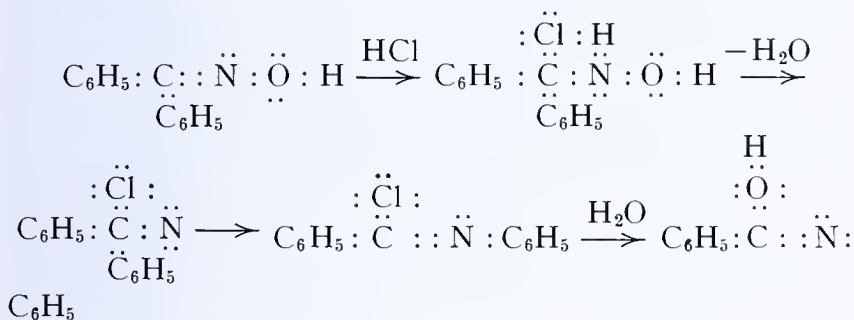


The methyl group then shifts to the univalent, highly unsaturated, nitrogen atom, and a pair of electrons shifts to give a double covalence between the nitrogen atom and the carbonyl group, forming methyl isocyanate,  $\text{CH}_3\text{:}\overset{\cdot\cdot}{\underset{\cdot\cdot}{\text{N}}}::\text{C}::\overset{\cdot\cdot}{\underset{\cdot\cdot}{\text{O}}}$ . Sodium hydroxide then hydrolyses the isocyanate to methylamine,  $\text{CH}_3\text{NH}_2$ , and sodium carbonate.

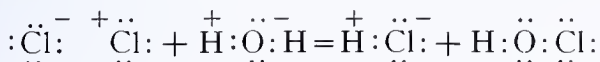
The reaction was not formulated in this way by Lengfeld and Stieglitz but the momentary formation of univalent nitrogen atoms was postulated by Stieglitz and his students in many reactions studied by them in later years. They never expressed these by means of electronic formulas, however.

In 1896 Stieglitz and his students began a study of the "Beckmann Rearrangement." He and his students continued the study of this rearrangement and of similar rearrangements of derivatives of triphenylmethylamine for more than twenty years. The work of this group furnished a major contribution to the solution of this difficult problem. One of their most important

suggestions was that of Stieglitz and Leech, J. Am. Chem. Soc. 36, (272, 607). When hydrochloric acid is the catalyst they assumed that this adds itself to the double union between carbon and the nitrogen of the oxime group, the chlorine uniting with the carbon and the hydrogen with the nitrogen as they are known to do in other similar cases. Water then separates from the nitrogen as it does from ammonium hydroxide. This leaves a univalent nitrogen atom to which one of the radicals combined with the carbon atom of the original ketoxime shifts as it does in many other cases studied by Stieglitz and his students. Hydrolysis of the chlorine atom combined with the carbon atom gives a hydroxyl compound tautomeric with the amide which is the final product of the rearrangement. The reactions for the oxime of benzophenone are as follows:



One of the most important theoretical suggestions made by Professor Stieglitz is found in an article published in 1901. After approving an article published by the author of this sketch in which an ionic reaction of molecular chlorine was assumed, he stated that he had presented similar views before students and professional colleagues in the University of Chicago. He cited the work of Jakowkin in which the latter demonstrated that chlorine water contains a mixture of molecular chlorine, hypochlorous acid and hydrochloric acid in equilibrium with each other. Professor Stieglitz explained this equilibrium as the result of a reversible reaction:



He emphasized in this paper the minimal formation of the positive chlorine ions, a fact not demonstrated till many years later.

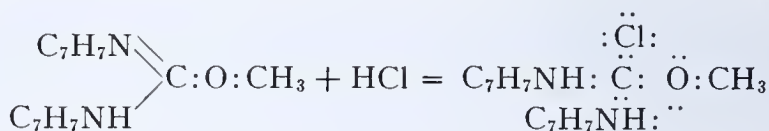
The paper implies that other covalent compounds may ionize

in a similar manner but this was not reconciled with the electronic theory of G. N. Lewis till 1923. (J. Am. Chem. Soc. 45, 2950.)

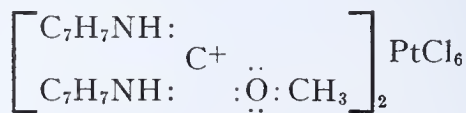
In 1903 Stieglitz published a paper on the theories of indicators. He showed that Ostwald's theory that a compound which is a weak acid and ionizes only slightly in a neutral or acid solution but ionizes to a large extent in the presence of hydroxyl ions which suppress the hydrogen ions, giving colored ions, is only a partial explanation of the conduct of an indicator. Ostwald failed to understand that the alkaline solutions cause a molecular rearrangement of the indicator giving deeply colored anions or, with some indicators such as methyl orange, give anions having a different color from that of the original indicator. Bernthsen had shown this relation ten years earlier but Ostwald had overlooked this.

Stieglitz explained the color as due to a quinoid structure due to a ketone or imido group in different indicators. His paper "Theories of Indicators" is a classic which furnishes a basis for all subsequent accurate discussions of this important topic.

In two papers published by Stieglitz and by Stieglitz and Dains in 1899, they came to the conclusion that the chlorine atom of hydrochloric acid adds to the carbon atom of an imido ester ("ether") while the hydrogen adds to the nitrogen.



The removal of a chloride ion from this compound to form the chloroplatinate ion,  $\text{PtCl}_6$ , would leave the carbon positive because it would have only three covalences, just as nitrogen with four covalences and no unshared electrons is positive. Dains prepared a chloroplatinate of this carbonium ion. This must have the structure,



At the time when these papers were written, electronic formulas for these compounds could not have been written.

Many chemists still fail to recognize that the six chlorine atoms of a chloroplatinate are united to the platinum atom with covalences and not in part to the univalent positive ion.

In 1904 Professor Stieglitz was asked to give one of the principal addresses at the International Congress of Arts and Sciences at St. Louis. His subject was "The Relation of Organic Chemistry to other Sciences." In this address he began his discussion of catalysis which was one of his major subjects of research for more than ten years.

During the nineteenth century a catalyst was supposed to be a substance which caused the acceleration of a reaction by its presence without combining with the reacting substances and without affecting the equilibrium involved in the reaction. Professor Stieglitz directed his attention primarily to the feature of acceleration and studied especially imido esters. The combination of the catalyst with the ester is involved and the older definition of catalysis was abandoned.

When Stieglitz took his degree in 1889 German chemists were mostly studying the structure of organic compounds, and physical chemistry was not thought to be an essential study in the training of a chemist. When Professor Stieglitz began the study of the catalysis of the hydrolysis of the imido esters he found that in order to understand clearly the mechanism of the reaction it would be necessary to use the methods of physical chemistry and the calculus, with which he was not familiar. He took the time and pains required to master so much of these subjects as was necessary for his purpose.

This study of catalysis furnished the basis for his study of esterification and saponification and went far to clarify the minds of chemists on these subjects.

Almost from the beginning of his instruction at the University, Dr. Stieglitz had charge of courses in analytical chemistry. His lectures were very carefully prepared and followed the general lines suggested by Ostwald's "*Wissenschaftliche Grundlagen der analytischen Chemie*."

After teaching the subject for many years he published in two volumes his "Qualitative Chemical Analysis" with the subtitle, "With special consideration of the laws of equilibrium and of the modern theories of solution." The book is a classic

and has been used in many of our best colleges and universities and is still so used.

The publishers state that there have been twenty printings. It has been almost the only text on the subject in China and it has been extensively used in Burma and India.

In 1915 Stieglitz published a paper on molecular rearrangements in which he followed J. J. Thomson's original electronic theory in which Thomson assumed that atoms are held together by the static attractions caused by the transfer of single electrons from one atom to another. This led Stieglitz to the use of such formulae as  $R_2N - + Cl$ , which are quite unsatisfactory now. It is surprising how closely he approached more modern views, and how helpful the Thomson theory proved to be as Stieglitz used it. His paper could be translated into modern electronic formulae by the use of the following principles recognized since his paper was written.

1. Positive charges of atoms and molecules reside exclusively in the nuclei of the atoms and negative charges in the electrons.
2. Atoms held together by a covalence may separate in two ways:
  - a. One electron may remain with each atom. The atoms remain electrically neutral.
  - b. The covalence pair may go with one of the atoms. This atom will be negative and the other positive.
3. A halogen atom held to nitrogen by a covalence usually separates in the positive form because the kernel of the nitrogen having only two electrons holds the covalence pair strongly. A halogen atom held to a phosphorus atom usually leaves it in the negative form carrying the covalence pair with it because the phosphorus kernel has ten electrons.\*

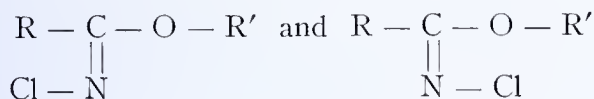
One of the most important parts of Stieglitz' theory is the formation of a momentary univalent nitrogen atom as an intermediate in the process. This has already been mentioned in discussing his work on the Beckmann rearrangement and illus-

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\* See Chem. Reviews 17, 19 (1935); J. Am. Chem. Soc. 51, 2392 (1929); Modern Alchemy p. 107.

trates how closely Stieglitz' researches were knit together and the care with which he worked out his ideas.

During the last years of the nineteenth century Hantsch explained the stereoisomerism of oximes by assuming a double union between the carbon and nitrogen atoms similar to the double union between two carbon atoms which causes the stereoisomerism of fumaric and maleic acids. In order to obtain independent support for this view Stieglitz and his students prepared several compounds of the type:



The stereoisomerism of these compounds was demonstrated by very careful and conclusive experiments.

It may be worth while to remark that the syn and anti groups which give the stereoisomerism in these cases are the chlorine atom and a pair of unshared electrons, while in the oximes the groups are the hydroxyl group and a pair of unshared electrons. Both hydroxyl and chlorine tend to separate from nitrogen in the positive, ephemeral and unstable form and, as Stieglitz has shown, chlorine atoms of this character do not readily rearrange unless the nitrogen also bears a hydrogen atom. It seems quite certain that the stereoisomerism is dependent on this fact.

In 1920 Professor Stieglitz gave an address before the Minnesota Section of the American Chemical Society on the "Theory of Color Production." The address as given at the Minnesota Section was not published but it was published in an abbreviated form in the Proceedings of the National Academy of Sciences in 1923 and in a more complete form in the Journal of the Franklin Institute in 1925.

In this address Stieglitz pointed out that the connection between the color of an organic compound and certain groups known as "chromophore" and "auxochrome" groups had long been known but that no attempt had been made to account for the action of these groups. He started by pointing out that color phenomena must be due to very short and, therefore, very rapid light waves. He concludes that these rapid waves must be due to electrons and not to atoms or molecules. He then

recalled that all dyes may be reduced to colorless, "leuco" compounds, a process which requires the addition of electrons, and that the leuco compound may, in turn, be oxidized, regenerating the dye by the removal of electrons. He considered that the ability of dyes to absorb light is due to the vibration of these mobile but restrained electrons. Apparently no one has given a better theory of the cause of the color of dyes.

Professor Stieglitz first took up the electron theory of valence on the basis of the theory of J. J. Thomson that atoms are held together by the transfer of single electrons from one atom to another, causing a static attraction between them. This would give a polar character to all chemical unions. In 1922 he published a paper on "The Electron Theory of Valence as Applied to Organic Compounds." In this paper he discusses carefully and at some length the advantages in favor of the view that organic compounds are held together by the static attraction between atoms one of which is electrically positive and the other electrically negative. The paper is still very valuable because in it Stieglitz cites numerous cases where the reactions of organic compounds indicate very clearly that many such compounds separate in such a manner as to show that one part of each is negative and the other positive. The following year the writer of this sketch published a short paper which reconciled these reactions with the theory of G. N. Lewis, *J. Am. Chem. Soc.* 45, 2959 (1923).

One hundred and eighteen dissertations for the degree of Ph.D. based on experimental work done under the direction of Professor Stieglitz were submitted to the University of Chicago from 1896 to 1936. In accordance with the custom of the University, abstracts of the experimental work were usually published in the chemical literature under the name of the student. Comparatively few of these papers can be found in the indexes listed under the name of Stieglitz.

More than eighty of these dissertations do not appear in any form in standard chemical literature. The theses were, however, filed in the Chemistry Library of the University of Chicago and are available for examination by individuals who are interested. The titles of these theses are listed in the bibliography appended to this biography.

KEY TO ABBREVIATIONS

- Am. Chem. J.—American Chemical Journal. Merged into Journal of the American Chemical Society, 1914  
 Ber.—Berichte der Deutsche Chemische Gesellschaft  
 Boston M. & S. J.—Boston Medical and Surgical Journal  
 Carnegie Inst. Pub.—Carnegie Institution of Washington Publications  
 Chem. Bull. Chicago—Chemical Bulletin, Chicago  
 Chem. Met. Eng.—Chemical and Metallurgical Engineering  
 Eighth Inter. Cong. App. Chem.—Eighth International Congress of Applied Chemistry  
 I. C. F. N.—International Catholic Federation of Nurses  
 Ind. Eng. Chem.—Industrial and Engineering Chemistry  
 J. Am. Chem. Soc.—Journal, American Chemical Society  
 J. A. M. A.—Journal, American Medical Association  
 J. Franklin Inst.—Journal, Franklin Institute  
 J. Ind. Eng. Chem.—Journal, Industrial and Engineering Chemistry  
 J. Pharmacol.—Journal of Pharmacology  
 N. Y. Med. J.—New York Medical Journal  
 Proc. Chem. Soc.—Proceedings, Chemical Society  
 Proc. Inst. Med. Chicago—Proceedings, Institute of Medicine, Chicago  
 Proc. Nat. Acad. Sci.—Proceedings, National Academy of Sciences  
 Sci. Mo.—Scientific Monthly  
 Trans. Am. Inst. Chem. Eng.—Transactions, American Institute of Chemical Engineers  
 Trans. Ill. State Acad. Sci.—Transactions, Illinois State Academy of Science  
 Univ. Chicago Mag.—University of Chicago Magazine

BIBLIOGRAPHY OF JULIUS STIEGLITZ

Including Ph.D. dissertations prepared by students working under his supervision.

1889

- Ueber die Kondensation der Säueramide und analoger Körper durch Abspaltung von Ammoniak oder dessen Derivaten. Inaugural Dissertation Universität zu Berlin, Dec. 14, 1889.  
 Ueber das Verhalten der Amidoxime gegen Diazobenzolverbindungen. Ber. 22, 3148-60.

1891

- On Benzoquinonecarboxylic Acids. Am. Chem. J. 13, 38-42.

1893-4

- Derivatives of Nitrogen Halogen Compounds. (With Felix Lengfeld.) Am. Chem. J. 15, 215-22 (1893); II *ibid.* 15, 504-18 (1893); III *ibid.* 16, 370-72 (1894).

1894

Ueber Alkylisoharnstoffe (Imidocarbaminsäureäther). (With Felix Lengfeld.) *Ber.* 27, 926-7.

The Action of Phosphorus Pentachloride on Urethanes. (With Felix Lengfeld.) *Am. Chem. J.* 16, 70-78.

1895

Note to article by E. E. Slosson, Ueber die Einwirkung von unterbromiger und unterchloriger Säure auf Säureanilide. *Ber.* 28, 3269-70.

On Imido-Ethers of Carbonic Acid. (With Felix Lengfeld.) *Am. Chem. J.* 17, 98-113.

Ueber die Einwirkung von Natriumäthylat auf Carbodiphenylimid. *Ber.* 28, 573-4.

Ueber Thiamine. (With Felix Lengfeld.) *Ber.* 28, 575-76.

Ueber Thiamine. (With Felix Lengfeld.) *Ber.* 28, 2742-44.

1896

On the "Beckmann Rearrangement". I. Chlorimidoesters. *Am. Chem. J.* 18, 751-61.

1897

Ph. D. Dissertation of Otto Folin. On Urethanes. *Am. Chem. J.* 19, 323-52.

1899

On the Constitution of the Salts of Imido Ethers and other Carbamide Derivatives. *Am. Chem. J.* 21, 101-11.

Ph. D. Dissertation of Frank Burnett Dains. On Isoarea Ethers and other Derivatives of Ureas. *J. Am. Chem. Soc.* 21, 136-92.

Ph. D. Dissertation of Herbert Newby McCoy. On the Hydrochlorides of Carbophenylimido Derivatives. *Am. Chem. J.* 21, 111-67. Preliminary reports, *Ber.* 30, 1090-94 (1897); *Ber.* 30, 1682-93 (1897).

Ph. D. Dissertation of Elizabeth Jeffreys. On Undecylamine and Penta-decylamine and the preparation of Higher Amines of the Alliphatic Series. *Am. Chem. J.* 22, 14-44. Preliminary report, *Ber.* 30, 898-901 (1897).

1900

Notes on Lecture Experiments to Illustrate Equilibrium and Dissocia-tion. *Am. Chem. J.* 23, 404-8.

Note on Nitrogen Halogen Compounds. (With E. E. Slosson.) *Proc. Chem. Soc.* 16, 1-2.

Ph. D. Dissertation of James Harvey Ransom. On the Molecular Re-arrangement of o-Aminophenylethylcarbonate to o-Oxyphenylurethane. *Am. Chem. J.* 23, 1-50. Preliminary report, *Ber.* 31, 1055-66 (1898).

1901

On Positive and Negative Halogen Ions. *J. Am. Chem. Soc.* 23, 797-99.  
Ph. D. Dissertation of Ralph Harper McKee. On the Oxygen Ethers of Ureas; Methyl and Ethylisoureas. *Am. Chem. J.* 26, 209-64. Preliminary papers, *Ber.* 32, 1494-96 (1899); 33, 807-12 (1899); 33, 1517-19 (1900).

1902

Book Review: The Elements of Physical Chemistry. By Harry C. Jones. Macmillan Co. N. Y., 1902. *J. Am. Chem. Soc.* 24, 588-91.

1903

The Theories of Indicators. *J. Am. Chem. Soc.* 25, 1112-27.  
On the "Beckmann Rearrangement" II. *Am. Chem. J.* 29, 49-68.  
The "Beckmann Rearrangement" III. Stereoisomeric Chlorimido Acid Ethers. (With Richard B. Earle.) *Am. Chem. J.* 30, 399-412.  
The "Beckmann Rearrangement" IV. *Am. Chem. J.* 30, 412-21.  
Ph. D. Dissertation of Edwin Emery Slosson. On Acylhalogenamine Derivatives and the Beckmann Rearrangements. *Am. Chem. J.* 29, 289-319. Preliminary papers, *Ber.* 28, 3265-69 (1895); *Ber.* 34, 1613-16 (1901).

1904

Ph. D. Dissertation of Henry Taber Upson. The Molecular Rearrangement of Aminophenylalkyl Carbonates, III. *Am. Chem. J.* 32, 13-43. I and II, *Am. Chem. J.* 31, 458-502.  
On the Beckmann Rearrangement. Decennial Publications of the University of Chicago, First Series, 9, 75-87.  
The Relation of Organic Chemistry to Other Sciences. *Proc. International Congress of Arts and Sciences, St. Louis, 1904, Vol. IV*, 276-84. Houghton, Mifflin and Co. (1906).  
The Constitution of Purpuric Acid and of Murexide. (With Max Slimmer.) *Am. Chem. J.* 31, 661-679.  
Book Review: Quantitative Analysis for Mining Engineers. By Edmund H. Miller. Van Nostrand Co., N. Y. 1904. *J. Am. Chem. Soc.* 26, 718-19.  
Ph. D. Dissertation of William McAfee Bruce. On the Oxygen Ethers of Urea. *J. Am. Chem. Soc.* 26, 419-36; 449-64.

1905

Book Review: Kritische Studien über die Vorgänge der Autoxydation. By C. Engler and J. Weissberg. Vieweg und Sohn, Braunschweig, 1904. *J. Am. Chem. Soc.* 27, 79-80.  
On Chloronium Salts. (With Edith E. Barnard.) *J. Am. Chem. Soc.* 27, 1016-19.  
Ueber die Isoharnstoffe. (With R. P. Noble.) *Ber.* 38, 2243-44.

1907

- Note on the article entitled, "Studies in Catalysis" by S. F. Acree. *Am. Chem. J.* 38, 743-46.
- Chemical Research in American Universities. Inaugural Address, University of Illinois, Oct. 18, 1907. *Science. New Series* 26, 699-703.
- Book Review: The Principles of Qualitative Analysis from the Standpoint of the Theory of Electrolytic Dissociation and the Law of Mass Action. By Wilhelm Böttger. P. Blakiston's Son and Company, Philadelphia, 1906. *J. Am. Chem. Soc.* 29, 1129-30.
- Ph. D. Dissertation of Louis Agassiz Test. Rearrangement of o-Aminophenyl Esters. No copy available.
- Ph. D. Dissertation of Robert Anderson Hall. Action of Ammonia on Isoeurea Esters. No copy available.
- Ph. D. Dissertation of Katherine Blunt. The Formation of Amidines. No copy available.
- Ph. D. Dissertation of Edith Ethel Barnard. Catalysis of Imido Esters as affected by the presence of non-electrolytes and Electrolytes. No copy available.

1908

- Studies in Catalysis, I: The Catalysis of Esters and of Amidoesters by Acids. *Am. Chem. J.* 39, 29-63.
- Studies in Catalysis, II: The Catalysis of Imidoesters. *Am. Chem. J.* 39, 166-83.
- Studies in Catalysis, III: The Theories of Esterification and Saponification. *Am. Chem. J.* 39, 402-31.
- The Theory of Indicators. *Am. Chem. J.* 39, 651-53.
- Note on the Solubility Product. *J. Am. Chem. Soc.* 30, 946-54.
- The "Syn" and "Anti" Stereoisomerism of Nitrogen Compounds. *Am. Chem. J.* 40, 36-46.
- The Application of Physical Chemistry to Organic Chemistry. (Address before the American Chemical Society, December, 1907.) *Science. New Series* 27, 768-75.
- Book Review: The Chemistry of Diazo-Compounds. By John Connell Cain. Edward Arnold. London, 1908. *J. Am. Chem. Soc.* 30, 1797-8.
- Book Review: Die Chemische Affinität und ihre Messung. By Otto Sackur. Vieweg und Sohn, 1908. *J. Am. Chem. Soc.* 30, 1795.
- Book Review: Laboratory Manual of Qualitative Analysis. By Wilhelm Segerblom. Longmans, Green and Co. 1908. *J. Am. Chem. Soc.* 1795-6.
- Book Review: Outlines of Qualitative Chemical Analysis. By Frank Austin Gooch. John Wiley & Sons, New York, 1906. *J. Am. Chem. Soc.* 30, 1796.
- Book Review: Qualitative Analysis von Standpunkte der Ionenlehre. By Wilhelm Böttger. Wilhelm Engelmann, Leipzig, 1908. *J. Am. Chem. Soc.* 30, 1796-7.

- Book Review: *Kurzes Lehrbuch der Organische Chemie*. By William A. Noyes. Akademisches Verlagsgesellschaft, Leipzig, 1907. *J. Am. Chem. Soc.* 30, 906-8.
- Book Review: *A Textbook of Organic Chemistry*. By A. F. Holleman. John Wiley & Sons, New York, 1907. *J. Am. Chem. Soc.* 30, 158-9.
- Book Review: *Organic Chemistry including Certain Portions of Physical Chemistry, for Medical, Pharmaceutical and Biological Students*. By H. D. Haskins and J. J. R. MacLeod. John Wiley and Sons, New York, 1907. *J. Am. Chem. Soc.* 30, 156-8.
- Ph. D. Dissertation of Ira H. Derby. *Studies in Catalysis IV. The Catalysis of Imidoesters*. *Am. Chem. J.* 39, 437-73. Preliminary paper, *A Study of Hydrolysis by Conductivity Methods*. *Am. Chem. J.* 37, 449-58 (1904).
- Ph. D. Dissertation of William McCracken. *Studies in Catalysis V. The Catalysis of Imidoesters*. *Am. Chem. J.* 39, 586-613.
- Ph. D. Dissertation of Hermann I. Schlesinger. *Studies in Catalysis VI. The Catalysis of Imidoesters*. *Am. Chem. J.* 39, 719-71.
- Ph. D. Dissertation of Willis Stose Hilpert. *Stereoisomeric Chlorimido Acid Esters*. *Am. Chem. J.* 40, 150-94.

1909

- The Relation of equilibrium between the Carbon Dioxide of the Atmosphere and the Calcium Sulfate, Calcium Carbonate and Calcium Bicarbonate of Water Solutions in Contact with it. *Carnegie Inst. Pub.* 107, 235-64.
- Ph. D. Dissertation of Peter Powell Peterson. *Stereoisomeric Chlorimido Ketones*. *Am. Chem. J.* 46, 325-44. Preliminary paper, *Ueber Stereoisomere Chlorimido-Ketone*. *Ber.* 43, 782-87 (1910).
- Ph. D. Dissertation of Lemuel Charles Raiford. *On Chlorimidoquinones*. *Am. Chem. J.* 46, 417-56.
- Ph. D. Dissertation of William Weldon Hickman. *The Catalysis of Imidoesters in the Presence of Acids*. (Typewritten.) Abstract in *Univ. Chicago, Science Series*, 1, 189-95 (1922-23).

1910

- Catalysis on the Basis of Work with Imidoesters*. *J. Am. Chem. Soc.* 22, 221-31.
- The Relation of Pure and Applied Science to the Progress of Knowledge and of Practical Affairs. 2. The Relation of Pure and Applied Chemistry. *Trans. Ill. State Acad. Sci.* 3, 62-7.
- Ph. D. Dissertation of John Colin Moore. *The Action of Water on Acylisoureas, etc.* (Typewritten.)
- Ph. D. Dissertation of Emma Perry Carr. *The Aliphatic Imido Esters*. (Typewritten.) Abstract in *Univ. Chicago, Science Series*, 1, 137-46 (1922-23).

1911

The Elements of Qualitative Analysis with Special Consideration of the Application of the Laws of Equilibrium and of the Modern Theories of Solution. 2 vols. Century Co., N. Y.

Ph. D. Dissertation of Le Roy Samuel Weatherby. The "Salt Effect" of Nitrates and Sulfonates in the Catalysis of Imido Esters. (Type-written.) Abstract in Univ. Chicago, Science Series 2, 179-87 (1923-4).

Ph. D. Dissertation of John Foote Norton. Simultaneous Reactions in Amidine Formation. (Typewritten.) Abstract in Univ. Chicago, Science Series, 5, 223-7 (1926-7).

Ph. D. Dissertation of Harlan Leo Trumbull. The Molecular Rearrangement of Acid Chloramides and the ionization of their Salts. (Type-written.)

1912

Catalysis on the Basis of Work with Imido Esters: The "Salt Effect." J. Am. Chem. Soc. 34, 1687-95.

1913

Catalysis on the Basis of Work with Imido Esters: A Contribution to the Problem of Saponification and Esterification. J. Am. Chem. Soc. 35, 1774-9.

Chairman's Address at the Willard Gibbs Medal Award to Dr. L. H. Baekeland, May 16, 1913. J. Ind. Eng. Chem. 5, 504-5.

Ph. D. Dissertation of George Oliver Curme, Jr. The Thermal Decomposition of Symmetrical Diarylhydrazines—A Reaction of the First Order. J. Am. Chem. Soc. 35, 1143-73. Preliminary paper, Ber. 46, 911-20 (1913).

Ph. D. Dissertation of Ethel Mary Terry (Mrs. McCoy). The Coefficient of Saponification of Ethyl Acetate by Sodium Hydroxide. J. Am. Chem. Soc. 49, 2216-22 (1927).

Ph. D. Dissertation of Paul Nicholas Leech, Jr. I. The Molecular Rearrangement of Triarylmethylhydroxylamines and the "Beckmann" Rearrangement of Ketoximes. J. Am. Chem. Soc. 36, 272-301, 607 (1914). II. A color Reaction of Hypochlorites with Methylaniline and Ethylaniline. J. Am. Chem. Soc. 35, 1042-44. Preliminary papers Eighth Inter. Cong. App. Chem. 25, 443 (1912); Ber. 46, 2147-51.

1914

Book Review: L'Additivité des Propriétés Diamagnetiques et son utilisation dans la Recherche des Constitutions. By Paul Pascal. A. Hermann et Fils, Paris, 1913. J. Am. Chem. Soc. 36, 456.

Ph. D. Dissertation of Bert Allen Stagner. Molecular Rearrangements of  $\beta$ -triphenylmethyl— $\beta$ -methylhydroxylamines and the Theory of Molecular Rearrangements. The Molecular Rearrangement of Triarylmethylhydroxylamines. Private edition distributed by Univ. Chicago Libraries, 1916. I. J. Am. Chem. Soc. 38, 2046-68 (1916); II. J. Am. Chem. Soc. 38, 2069-81 (1916).

JULIUS STIEGLITZ—NOYES

Ph. D. Dissertation of Agnes Fay Morgan. I. The Molecular Rearrangements of Some Triarylmethylchloroamines. II. The Viscosities of Various Methyl- and Ethylimido-benzoate Salts and of *Para*- and *Meta*-nitrobenzoylchloramides in moderately concentrated Aqueous Solutions. (Typewritten.) Abstract in Univ. Chicago Science Series, 1, 213-17 (1922-23). Part I, J. Am. Chem. Soc., 38, 2095-2101 (1916).

1915

Molecular Rearrangements of Triphenylmethane Derivatives. I. General Discussion. Proc. Nat. Acad. Sci. 1, 196-202.

Molecular Rearrangements of Triphenylmethane Derivatives. II. Experimental Part. (With Collaborators.) Proc. Nat. Acad. Sci. 1, 202-10 (1915).

Presentation Address: Willard Gibbs Medal Award to Arthur A. Noyes, April 16, 1915. J. Ind. Eng. Chem. 7, 449-50.

John Ulric Nef. Univ. Chicago Mag. 8, 17-18.

1916

Ph. D. Dissertation of Helen Treadway. The Thermal Decomposition of Symmetrical Diarylhydrazines. Private edition distributed by Univ. Chicago Libraries, 1916. J. Am. Chem. Soc. 38, 1736-60.

Ph. D. Dissertation of Townes Randolph Leigh. I. Chlorimidoesters. II. Chlorimidobenzil. (Typewritten.) Private edition distributed by Univ. Chicago Libraries, 1922.

Life and Work of John Ulric Nef. Trans. Ill. State Acad. Sci., 9, 18-19.

The Oxidation of Carbohydrates. Proc. Inst. Med. Chicago 1, 41-50.

Ph. D. Dissertation of Issabelle Marion Vosburgh (1911-13). The Molecular Rearrangement of Triphenylmethylhalogen Amines. Private edition distributed by Univ. Chicago Libraries. J. Am. Chem. Soc. 38, 2081-95. Abstract, Eighth Inter. Cong. App. Chem. 25, 445 (1912). Ber. 46, 2151-56 (1913).

1917

Address at opening session Kansas City Meeting, American Chemical Society. J. Ind. Eng. Chem. 9, 443-46.

American Chemical Industry and the American Chemical Society. (Third Exposition of Chemical Industries.) J. Ind. Eng. Chem. 9, 1005-6.

The Outlook in Chemistry in the United States. J. Am. Chem. Soc. 39, 2095-2110. Science, 46, 321-333.

War Service of Chemists. (With Charles L. Parsons.) Editorial, J. Ind. Eng. Chem. 9, 730.

Sigma Xi's Opportunity. Sigma Xi Quarterly, 5, 49-53.

Why the Nation Needs Chemistry. Independent 92, 90.

Ph. D. Dissertation of Sidney Marsh Cadwell. I. The Effect of Acid on the Speed of Rearrangement of the Benzene Sulphonyl Ester of Benzophenoneoxime. II. The Quantitative Determination of very Small Amounts of Gold, Especially in Animal Tissues. (Typewritten.) Abstract in Univ. Chicago, Science Series, 1, 131-36 (1922-23).

- Ph. D. Dissertation of Adeline Mae De Sale (Mrs. Link). The Velocity of rearrangement of Benzoyl Azide. (Typewritten.) Abstract in Univ. of Chicago Science Series, *I*, 163-7 (1922-3).
- Ph. D. Dissertation of James Kuhn Senior. The Molecular Rearrangement of Triarylmethylazides. The Molecular Rearrangement of Sym.-bis-triarylmethylhydrazines. Private edition distributed by Univ. Chicago Libraries, 1917. J. Am. Chem. Soc. *38*, 2718-26: 2728-36 (1916). Preliminary report, Proc. Nat. Acad. Sci. *I*, 207-10 (1915).
- Ph. D. Dissertation of Ralph Lyman Brown. The Molecular Rearrangement of Sym.-bis-triphenylmethylhydrazine. Private edition distributed by Univ. Chicago Libraries, 1922. J. Am. Chem. Soc. *44*, 1270-92 (1922).

1918

- The Problem of the Synthetic Drug. Abstract of Talk before 69th Annual Meeting of American Medical Association, Chicago, June 10, 1918, N. Y. Med. J. *107*, 1193.
- Synthetic Drugs, I. J. A. M. A. *70*, 536-7; II. *ibid.* 688-9; III. *ibid.* 923.
- Procaine and Novocaine Identical (A letter to the Editor). Boston M. and S. J. *178*, 916.
- Synthetic Pharmaceuticals and the Patent Law. Trans. Am. Inst. Chem. Eng. *11*, 369-82.
- The Preparation of Phenylethylbarbituric Acid. (With Mary Rising). J. Am. Chem. Soc. *40*, 723-30.
- New Chemical Warfare. Yale Review, *7*, 493-511.
- Henry Shaler Williams and the Society of Sigma Xi. Sigma Xi Quarterly, *6*, 68-69.
- Letter to the Chairmen and Secretaries of the Sections of the American Chemical Society, on "The Reduction of Waste." Dec. 22, 1917. J. Ind. Eng. Chem. *10*, 153.
- Letter to Mr. H. LeB. Gray on "The Reduction of Waste", Dec. 27, 1917. J. Ind. Eng. Chem. *10*, 153.
- Ph. D. Dissertation of Harry Clyde Trimble. The Effect of Electric Currents in the Attempt to Induce Molecular Rearrangements. (Typewritten.) Abstract in Univ. Chicago Science Series, *I*, 257-62 (1922-3).
- Ph. D. Dissertation of Denton Jacobs Brown. The Velocity of Decomposition of Benzoyl Peroxide. An Electroanalytical Method for the Determination and Separation of the Metals of the Copper-Tin Group. (Typewritten.) Abstract in Univ. Chicago Science Series, *3*, 101-4 (1924-5).
- Ph. D. Dissertation of Ralph Emory Nelson. The Velocity of Saponification of Ethyl Acetate by Sodium Hydroxide under Conditions of Varying Concentrations of Ester and Base at 25° and 18°. (Typewritten.)

1919

- Samuel Wendell Williston. In the Society of Sigma Xi. Sigma Xi Quarterly, *7*, 22-25.

- Report of the Committee on War Service for Chemists. *J. Ind. Eng. Chem.*, *11*, 413-15.
- Report of the Committee on Publication of Compendia of Chemical Literature, etc. *J. Ind. Eng. Chem.*, *11*, 415-17.
- Report of the Committee on Cooperation between the Universities and the Industries. *J. Ind. Eng. Chem.*, *11*, 417.
- Chemistry and Medicine; a Tribute to the Memory of John Harper Long. *Science*, *49*, 31-38. *Proc. Inst. Med.*, Chicago, *2*, 124-33 (1918-19).
- Back to the Colleges and Universities. (Letter to the Editor.) *J. Ind. Eng. Chem.*, *11*, 378.
- Introduction to book: Creative Chemistry, by Edwin E. Slosson. Garden City Publishing Company, New York.
- Ph. D. Dissertation of Henry John Rossbacher. *m*-Tolylethyl-barbituric Acid. (Typewritten.)

1920

- A Comment from Dr. Julius Stieglitz. *Chem. Bull.*, Chicago, *1*, 103-4.
- William Albert Noyes. In Appreciation. *J. Ind. Eng. Chem.* *12*, 106.
- President's Address. Twentieth Convention of Sigma Xi, Dec. 30, 1919. *Sigma Xi Quarterly*, *8*, 3-9.
- Ph. D. Dissertation of Mary M. Rising (Mrs. Julius Stieglitz). I. The Preparation of Phenylethylbarbituric Acid. II. The preparation of *p*-Ureido-phenylacetylurea and related Compounds. III. An Attempt to filter the Enzymes of Milk. Private edition distributed by Univ. Chicago Libraries, 1920. I. *J. Am. Chem. Soc.*, *40*, 723-30 (1918). II. *J. Am. Chem. Soc.* *42*, 128-36. III. (With Jean Piccard) *J. Am. Chem. Soc.* *40*, 1275-81 (1918).
- Ph. D. Dissertation of Herman Vance Tartar. The Constitution of Murexide and the Theory of Dyes. (Typewritten.)
- Ph. D. Dissertation of John Edward Schott. I. Oxidation and Rearrangement of Benzamide. II. Derivatives of Phenylethylbarbituric Acid. (Typewritten.)
- Ph. D. Dissertation of Frank Louis Debeukelaer. Urea Derivatives of Phenylethylacetic Acid. (Typewritten.) Abstract in *Univ. of Chicago Science Series*, *1*, 155-61 (1922-3).
- Ph. D. Dissertation of Steward Basterfield. Derivatives of Ethylisourea Ether and their Pharmacological Properties. (Typewritten.) Printed in part in *J. Pharmacol.*, *20*, 451-61 (1923).

1921

- Presentation of the Willard Gibbs Medal to Madame Curie. *Chem. Met. Eng.* *24*, 1137.
- The Theory of Color Formation. Address given at the 90th meeting of the Minnesota Section of the American Chemical Society, Dec. 3, 1920. *J. Am. Chem. Soc.* *43*, Proc. 36.
- Preparation of 5,5'-Mercuri-bis-3-nitro-4-hydroxy-phenyl-arsonic Acid (With Morris Kharasch and Martin Hanke). *J. Am. Chem. Soc.* *43*, 1185-93.

- Ph. D. Dissertation of Martin Edward Hanke. An Attempt to Prepare a Mercury Derivative of Dioxydiaminoarsenobenzene (Arsphenamine) (Typewritten).
- Ph. D. Dissertation of George Ross Robertson. Arsenical Derivatives of Phenylacetic Acid. (Typewritten). *J. Am. Chem. Soc.*, 43, 179-81; 182-5.
- Ph. D. Dissertation of James Hollingsworth Clemmer Smith. (Type-written.) I. Arsenic Derivatives of Phenylaminoacetic Acid. II. The Estimation of Sodium Hyposulfite. Abstract in *Univ. Chicago Science Series*, 1, 231-4 (1922-3); II. *J. Am. Chem. Soc.* 43, 1307-8.
- 1922
- The Electron Theory of Valence as Applied to Organic Compounds. *J. Am. Chem. Soc.* 44, 1293-1313.
- Addendum to above, *J. Am. Chem. Soc.* 44, 1833-4.
- Ph. D. Dissertation of Lyman Chalkley, Jr. I. The Mercurization of Methylene Blue. II. The Mercurization of some substituted Phenyl Ammonium Salts and Anilines. III. Nitrobenzene Mercury Compounds, an indirect Method of Mercurizing Organic Compounds. Abstract in *Univ. Chicago Science Series* 1, 147-53 (1922-3). I. *J. Am. Chem. Soc.* 47, 2055-61 (1925).
- Ph. D. Dissertation of Quaesita Cromwell Drake. An Investigation of the Possibility of Rearranging an Acyl- $\beta$ -alkyl-hydroxylamine. (Type-written.) Abstract in *Univ. Chicago Science Series*, 1, 169-75 (1922-3).
- Ph. D. Dissertation of Earl C. Gilbert. Molecular Rearrangements of Hydrazine Derivatives. IV. The Rearrangement of Benzoylhydrazine, Dibenzoylhydrazine, and Azodibenzoyl. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 1, 177-82 (1922-3).
- Ph. D. Dissertation of Arthur Preston Locke. Arsenical Derivatives of Phenylacetic Acid, With a discussion of a Theory of Diazotization and Syntheses by means of Diazo Compounds. II. The potentiometric Estimation of Arylamines. (Typewritten). Abstract in *Univ. Chicago Science Series*, 1, 203-6 (1922-3).
- Ph. D. Dissertation of Julian Francis Smith. Rearrangement of Hydrazine Derivatives. III. Rearrangement of Benzophenonehydrazone and of Triphenylmethylhydrazine. (Typewritten.) *Univ. Chicago Science Series*, 1, 235-9 (1922-3).
- Ph. D. Dissertation of James Kidder Stewart. The Molecular Rearrangement of Diphenylmonobiphenylmethylchloroamine. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 1, 247-52 (1922-3).
- Ph. D. Dissertation of Stewart Duffield Swan. The Velocity of Saponification of Ethyl Formate by Trimethylammoniumhydroxide in the presence of Trimethylammonium Salts. Abstract in *Univ. Chicago Science Series*, 1, 253-6 (1922-3).

JULIUS STIEGLITZ—NOYES

1923

- A Theory of Color production I; II. Inorganic Compounds. *Proc. Nat. Acad. Sci.* 9, 303-12.
- The future Independence and Progress of American Medicine in the Age of Chemistry (With others). 80 pp. The Chemical Foundation, Inc. New York.
- Ph. D. Dissertation of Archibald Turner McPherson. The Electromerism of Bromine Chloride. I. The addition of Mixtures of Bromine and Chlorine to Cinnamic Acid. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 1, 207-11 (1922-3).
- Ph. D. Dissertation of Marion Gabriel Frank. Experiments to Prepare Mercury Derivatives of Methylene Blue. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 2, 113-7 (1923-4).
- Ph. D. Dissertation of Leslie Hellerman. A study of Rearrangements of Halogenamine Derivatives in Anhydrous Media. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 2, 125-9 (1923-4).
- Ph. D. Dissertation of Mary Lura Sherrill. I. Reaction of Bromonitromethane with Aromatic Compounds in the presence of Aluminum Chloride—a Case of Electromerism. II. The preparation of Pure Methylene-di-salicylic Acid. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 2, 171-8 (1923-4). *I. J. Am. Chem. Soc.* 46, 2753-58 (1924).
- Ph. D. Dissertation of Edward Staunton West. A Study of the Molecular Rearrangements of Sym.-bis-triphenylmethylhydrazine and Mono-triphenylmethylhydrazine. Abstract in *Univ. Chicago Science Series*, 2, 189-94 (1923-4).

1924

- Ph. D. Dissertation of Chang-Kong Chuang. Stereoisomeric Hydrazone,  $RR'C:N.NH_2$ . (Typewritten.) Abstract in *Univ. Chicago Science Series*, 3, 111-118 (1924-5).
- Ph. D. Dissertation of Barclay Lincoln Jones. Derivatives of Nile Blue A. On the Mercurization of Nile Blue A. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 3, 143-48 (1924-25).
- Ph. D. Dissertation of Margaret Fitch Wilcox McPherson. Derivatives of Phenyl-butyric Acid. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 3, 155-61 (1924-5).
- Ph. D. Dissertation of Karl Stone Means. A Study of the Velocity Constant for the Saponification of Ethyl Formate. (Typewritten.) Abstract in *Univ. Chicago Science Series* 3, 163-9 (1924-5).

1925

- Chemistry in the Service of Man. 23 pp. A Booklet published by the Committee on the Development of the University of Chicago. *Univ. Record*, 11, 1-12.

- A Theory of Color Production. (Address, at the Centenary of the Franklin Institute, Sept. 1924.) *J. Franklin Inst.* 200, 35-49.
- Ph. D. Dissertation of William Gustav Friedemann. I. Velocity of Saponification of Methyl Formate at 25°. II. The Hydrolysis of Sodium Cyanide, Potassium Cyanide and Barium Aspariginate at 25°. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 4, 81-6 (1925-6).
- Ph. D. Dissertation of Israel Morris Levine. The Mercuration of Neutral Red and its Sulfonic Acid. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 4, 105-10 (1925-6).
- Ph. D. Dissertation of Andrew McNally Neff. The Preparation and Rearrangement of Methylphenylmethylchloroamine. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 4, 141-9 (1925-6).

1926

- Chemistry and Recent Progress in Medicine. Dohme Lectures, 1924, pp. viii, 62, Williams and Wilkins Company, Baltimore, 1926.
- Ph. D. Dissertation of Joseph Solomon Friedman. The Mercuration of Nile Blue. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 5, 171-5 (1926-7).
- Ph. D. Dissertation of Elbert Dung Wui Ho. The Action of Phenylhydrazine on Bromohydrouracil—an Attempt to Prepare 5-Aminohydrouracil. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 5, 183-8 (1926-7).
- Ph. D. Dissertation of Mary Eugenie Maver. The Preparation, Rearrangement and Reduction of  $\beta$ -Methylphenylmethylhydroxylamine. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 4, 111-6 (1925-6).
- Ph. D. Dissertation of John Demetrius Xanthopoulos. Rearrangement of Stereoisomeric Hydrazones. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 4, 195-201 (1925-6).
- Ph. D. Dissertation of Hoylande Denune Young. Stereoisomeric Bromoimino Ketones. (Typewritten.) Abstract *Univ. Chicago Science Series*, 4, 203-7 (1925-6).

1927

- Ph. D. Dissertation of Mabelle Crystale Dame. I. The Preparation and Rearrangement of Phenylbiphenylmethylchloroamines. II. An Attempt to Effect Rearrangement of Triphenylmethyl- $\beta$ -methylhydroxylamine by heat. (Typewritten.) Abstract in *Univ. Chicago Science Series*, 5, 163-9 (1926-7).
- Ph. D. Dissertation of Harold Lawrence Mason. A Study of Tautomeric Chlorimidophenols; A Contribution to the Theory of Color Production in Dyes. (Typewritten.) *Univ. Chicago Science Series*, 5, 199-203 (1926-7).

- Ph. D. Dissertation of Irving Elkin Muskat. The Mechanism of Substitution in the Aromatic Nucleus. Direct Addition in the Benzene Ring. (Typewritten.) Abstract in Univ. Chicago Science Series, 5, 217-21 (1926-7).
- Ph. D. Dissertation of Walter Charles Russell. The Electromerism of Bromine Chloride. II. The Reaction of a Bromine-Chlorine Mixture with Diphenylketene. (Typewritten.) Abstract in Univ. Chicago Science Series, 5, 233-8 (1926-7).
- Ph. D. Dissertation of James Bernard Culbertson. A Study of Tautomerism of Aromatic Ketimines. (Typewritten.) Abstract in Univ. Chicago Science Series, 6, 135-42 (1927-8).

1928

- The Department of Chemistry. Univ. Record, New Series, 14, 236-9.
- Ph. D. Dissertation of Arthur Edward Remick. The Velocity of Saponification of Methyl and Ethyl Formates at 25°. (Typewritten.) Abstract in Univ. Chicago Science Series, 6, 171-9 (1927-8).
- Ph. D. Dissertation of Dell S. Garby. Arsenic Derivatives of Phenylaminoacetic Acid. II. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 159-63 (1928-9).
- Ph. D. Dissertation of Luther Orland Leach. The Relative Polarity of the Halogens in the Halo-Acetic Acids. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 171-6 (1928-9).
- Ph. D. Dissertation of Dorothy Virginia Nightingale. Studies in the Murexide and Alloxantine Series. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 177-82 (1928-9).
- Ph. D. Dissertation of Willis Kuhn Weaver. Rearrangements of Methyl-diphenylmethylmonochloroamine and Methyl-diphenylmethylazide. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 203-8 (1928-9).
- Ph. D. Dissertation of Oden Elbridge Sheppard. An Attempt to Prepare Tautomeric Indophenols. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 197-202 (1928-9).

1929

- The Past and the Present. Univ. Record, 15, 67-72.
- Address: Priestly Medal Award to Francis P. Garvan. Ind. Eng. Chem. 21, 897.
- As Editor, Chemistry in Medicine, The Chemical Foundation, Inc. 1928, 757 pp., 25 illustrations. Chemical Publishing Co., N. Y.
- Ph. D. Dissertation of Maurice Leon Cohn. Rearrangements of Dimethyl-phenylmethyl-dichloroamine, Dimethylphenylmethylmonochloroamine, and  $\beta$ -Dimethylphenylmethylhydroxylamine. (Typewritten.) Abstract in Univ. Chicago Science Series 8, 71-5 (1929-30).

- Ph. D. Dissertation of John Elton Cole. The Product of the Action of Phenyl Magnesium Bromide on Acetophenone oxime:  $\beta$ ,  $\beta$ -diphenyl- $\beta$ -hydroxyethylamine. (Typewritten.) Abstract in Univ. Chicago Science Series, 7, 145-50 (1928-9).
- Ph. D. Dissertation of William Elias Sturgeon. I. A Study of the Product of the Action of Phenyl Magnesium Bromide on Propiophenoneoxime: 1, 1-diphenyl-1-hydroxy-2-aminopropane. II. A Further Study of the Action of Phenyl Magnesium Bromide on Acetophenoneoxime: 1, 1-diphenyl-1-hydroxy-2-aminoethane. Abstract in Univ. Chicago Science Series, 8, 145-51 (1929-30).

1930

- Laboratory Courses by Correspondence. National Univ. Extension Assoc. Proc. 15, 132-3.
- A Comment on Home Study. National Univ. Extension Assoc. Proc. 15, 38-9.
- Ph. D. Dissertation of Ferris Bertrand Crum. Studies in Hydrolysis of Nitrogen Chloride and Nitrogen Bromide Derivatives (Typewritten). Abstract in Univ. Chicago Science Series, 9, 41-47 (1930-2).
- Ph. D. Dissertation of Charles William Lenth. The Molecular Rearrangement of  $\beta$ ,  $\beta$ -diphenyl- $\beta$ -hydroxy-ethylamine. Abstract in Univ. Chicago Science Series, 8, 101-7 (1929-30).
- Ph. D. Dissertation of Frederick Madison Meigs. The Action of Organic Halides on the Alkali Metal Salts of the Alcohols (Typewritten). Univ. Chicago Science Series, 8, 125-31 (1929-30).
- Ph. D. Dissertation of Minette Dorothea Newman. Mercuration in the Triphenylmethane Series of Dyes. Mercuration of Aurin. (Typewritten). Abstract in Univ. Chicago Science Series, 8, 133-8 (1929-30).
- Ph. D. Dissertation of Frederick Charles Maxted Smithson. Reactions of Hypochlorous Acid on Substituted Aromatic Amines, and on Cinnamic and Crotonic Acids. (Typewritten). Abstract in Univ. Chicago Science Series, 8, 139-44 (1929-30).
- Ph. D. Dissertation of Harold Herbert Tucker. A Study of 2-Chloro-3-bromindophenol. (Typewritten). Abstract in Univ. Chicago Science Series, 8, 153-7 (1929-30).
- Ph. D. Dissertation of Ruth Maude Watts. Studies in the Introduction of the Arsonic Group ortho to the Amino group in Benzene Derivatives. (Typewritten). Abstract in Univ. Chicago Science Series, 9, 81-7 (1930-2).
- Ph. D. Dissertation of Russell Blauch Cooper. The Pinacol-Pinacolone Molecular Rearrangement. (Typewritten). Trans., Ill. State Acad. Sci. 25, 173 (1933). Abstract in Univ. Chicago Science Series, 8, 77-82 (1929-30).

1931

- The Role of Chemistry in Medicine. The Courier of I. C. F. N., 3, No. 9; 15-18.

- Ph. D. Dissertation of John Benjamin Cloke. I. The Formation of Pyrrolines from  $\gamma$ -Chloropropyl and Chloropropyl Ketimines. II. A Study of the Relationship between the Structures of Cyclopropyl Ketimines, their Stabilities in Water, and their Strengths as Bases. (Typewritten.) Abstract in Univ. Chicago Science Series, 9, 33-40 (1930-2). I. J. Am. Chem. Soc., 51, 1174-87 (1929).
- Ph. D. Dissertation of John James Hoffmann. The Theory of Substitution in Benzene Derivatives. The Action of nitrous acid on Phenolic Ethers. (Typewritten.) Abstract in Univ. Chicago Science Series, 9, 49-55 (1930-2).
- Ph. D. Dissertation of Agnes Ethyl Sharp. A Study of the 1, 2-Dihalo-phenylethanes. (Typewritten.) Abstract in Univ. Chicago Science Series, 9, 67-71 (1930-2).
- Ph. D. Dissertation of Spencer Gordon Stoltz. Molecular Rearrangements of Alkylarylmethylamine Derivatives. (Typewritten.) Abstract in Univ. Chicago Science Series, 9, 73-80 (1930-2).
- Ph. D. Dissertation of George Washington Ayers, Jr. The Pinacol-Pinacolone Rearrangement; The Rearrangement of the Tetramethylene Halohydrines. (Typewritten.) Brief account by Julius Stieglitz, Russell B. Cooper and George W. Ayers, Jr. Trans. Ill. State Acad. Sci., 25, 173 (1933).

1932

- Ph. D. Dissertation of Kenneth Howard Adams. The Pinacol-Pinacolone Molecular Rearrangement. I. The Rearrangement of Pinacol and of Tetramethylethylenechlorohydrin. II. The Rearrangement of  $\beta$ -Aminotetramethylethanol. (Typewritten.)
- Ph. D. Dissertation of Benjamin Leo Maizel. A Study of the Interaction of Cyanogen Bromide and unsaturated Compounds, Especially of the Enolic Type. (Typewritten.)
- Ph. D. Dissertation of Kenneth Nielsen Campbell. A Study of the Action of the Grignard Reagent on Aromatic Oximes. (Typewritten.) Litho-printed in a private edition distributed by Univ. Chicago Libraries, 1935.
- Ph. D. Dissertation of Raymond Wright Johnson. The Velocity of hydrolysis of Stereoisomeric Hydrazones and Oximes. (Typewritten.) Private edition distributed by Univ. Chicago Libraries, 1934, also J. Am. Chem. Soc., 56, 1904-8 (1934).
- Ph. D. Dissertation of Walter Sigmund Guthmann. I. Molecular Rearrangement of N-Triphenylmethyl-O-alkyl Hydroxylamines. J. Org. Chem. 1, 31-7 (1936). II. The interaction of Ether and Phosphorus Pentachloride. J. Am. Chem. Soc., 54, 2938-40.

1933

- Chemistry and Recent Medical Progress (Science Service Radio Talk). Sci. Mo., 37, 451-3.
- Chemical Studies of the Mechanism of the Narcosis Induced by Hynotics. I. The Synthesis of Colored Derivatives of Phenobarbital. (With Mary M. Rising and John R. Shroyer). J. Am. Chem. Soc., 55, 2817-20.

- Richard Willstätter, Leader in Organic Chemistry (An Address). Chem. Bull., Chicago, 20, 173-7.
- The Nature of Chemical Processes. Chapter 5, pp. 117-169, The Nature of the World and of Man. Edited by H. H. Newman. Garden City Publishing Company, Inc., New York, 1933.
- Ph. D. Dissertation of Eleanor Rachel Bartholomew. Some Substituted Pyrimidines. (Typewritten.) Lithoprinted, Univ. Chicago Libraries, 1936.
- Ph. D. Dissertation of Paul Vernon Brower. The Pinacol-Pinacolone Rearrangement; The Rearrangement of 2-Phenylaminotetramethyl-ethanol. (Typewritten.) Lithoprinted in private edition, distributed by Univ. Chicago Libraries, 1936.
- Ph. D. Dissertation of Rachel Fuller Brown. The Molecular Rearrangement of *p*-Phenylbenzobromamide. (Typewritten.)
- Ph. D. Dissertation of Gordon Hamilton Stillson. Further Studies on the Molecular Rearrangement of  $\beta$ ,  $\beta$ -Diphenyl- $\beta$ -hydroxyethylamine. (Typewritten.) Lithoprinted in a private edition distributed by the Univ. Chicago Libraries. 1936.
- Ph. D. Dissertation of Calvin Stockton Yoran. The Sulfonation of 5-Phenyl-5-ethylbarbituric Acid. (Typewritten.)

1934

- Ph. D. Dissertation of Frederick H. Roberts. The Pinacol-Pinacolone Rearrangement; the Rearrangement of Pinacol Dibromide. (Typewritten).

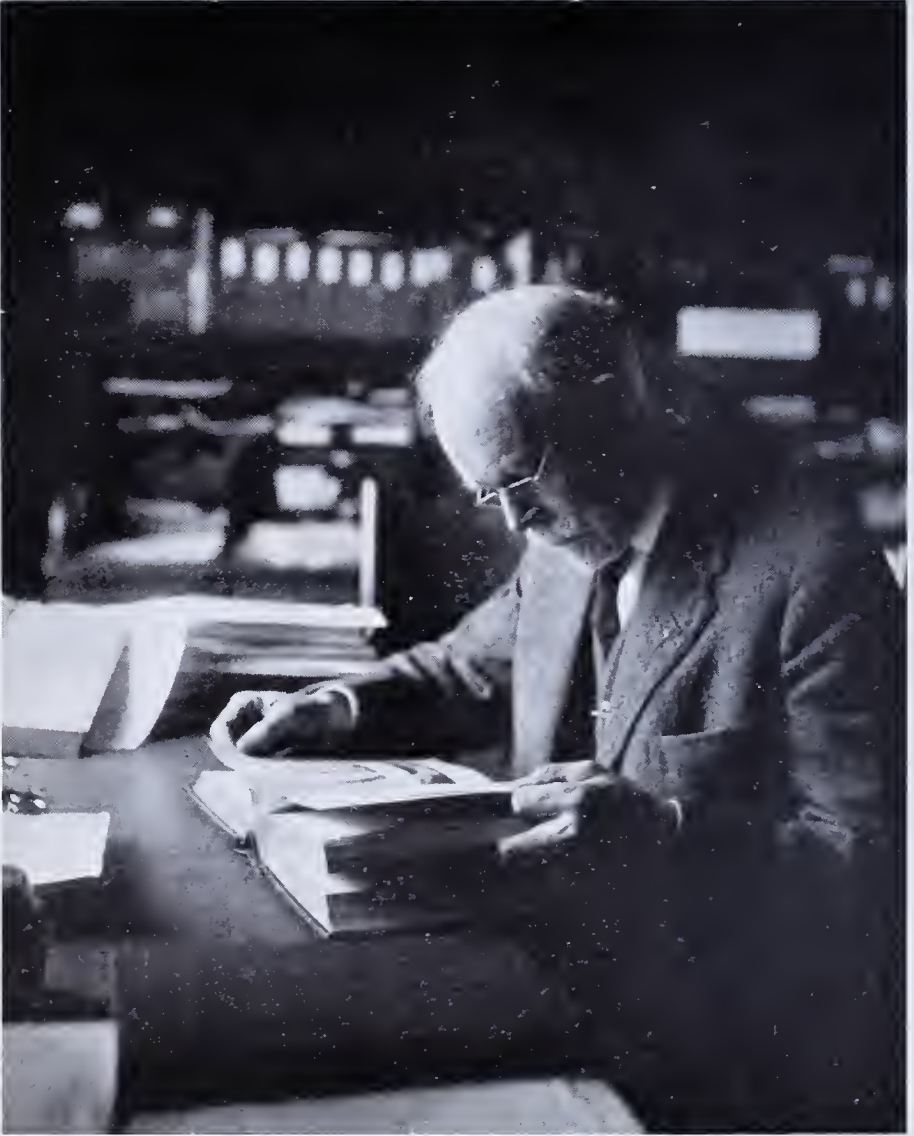
1935

- Herbert Newby McCoy. A Brief Biography. Ind. Eng. Chem. News Ed., 13, 280.
- Ph. D. Dissertation of Philip John Elman. The Molecular Rearrangement of 1, 1, 1-Dimethylhydroxy-2, 2-diethyl-2-amino ethane. (Typewritten.)
- Ph. D. Dissertation of Yih-Tong Ku. Substitution Reactions of Derivatives of Nitrobenzene. (Typewritten.)
- Ph. D. Dissertation of Earl Richard Tweedle. Some Substituted Pyrimidines. (Typewritten.)

1936

- Ph. D. Dissertation of Herman Samuel Bloch. The Molecular Rearrangement of 2, 2-Diphenyl-2-hydroxy-ethylamine. (Typewritten.)
- Ph. D. Dissertation of Rex Everett Lidov. Studies toward the Synthesis of 4-Methyl-5, 5-diethyl uracil. (Typewritten.)
- Ph. D. Dissertation of Julia Lurena Southard. Urea Derivatives of Some Imino Compounds. (Typewritten.)
- Ph. D. Dissertation of Nathaniel Morton Winslow. A Study of the Structure of the Murexides and the Alloxantines. (Typewritten.)





Edmund B. Wilson .

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OF

EDMUND BEECHER WILSON

1856-1939

BY

T. H. MORGAN

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PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1940

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## EDMUND BEECHER WILSON

1856-1939

BY T. H. MORGAN

Wilson has written in regard to his ancestry, "I will frankly confess that I am proud of certain things in my ancestry; first, because it is purely American back to 1620; second, because it is purely New England and especially Cape Cod; and third, because so many of my forefathers were seafaring men".

"I have always loved the sea and all its ways—the ships—the men who sailed them—the strange beasts that make their home in it. I am descended from Thomas Clarke, reputed mate of the *Mayflower*—who settled in Plymouth in 1623". His eldest son, Andrew Clarke, removed to Boston, later to Harwich. Wilson's maternal grandfather, Scotto Clarke, born at Harwich in 1782, married in 1809 and later moved to Boston. After the financial crash of 1837 he, with his four children, Harriet, Charles, Caroline, (Wilson's future mother) and Ellen, migrated to Geneva, Illinois. Wilson's father's ancestors came originally from Wrentham, Massachusetts. His father, Isaac G. Wilson, went through Brown University, graduating in 1838, then attended the Harvard Law School and was admitted to the Massachusetts Bar in 1841. In the following year he began the practice of law at Elgin, Illinois, ten miles up the valley of the Fox River and ten miles from Geneva. Afterwards he moved to Geneva where in 1843 he married Caroline Clarke, the second daughter of Scotto Clarke. Later Wilson's father became county judge, circuit judge and chief justice.

Edmund Beecher Wilson was born October 19, 1856, at Geneva, Illinois. The first sixteen years of his life were passed there. He has written "it would not be easy to imagine a happier environment for a boy who somehow managed to combine an early passion for natural history with an almost equal love for music; who grew up in an atmosphere of warm affection and sympathetic understanding at home, and was surrounded by a circle of intelligent and cultivated people".

When only two and a half years of age, "Eddy," as he was called, was adopted by his mother's sister, Mrs. Charles Patten,

who also lived in Geneva; "with the result that henceforward I had in effect two homes and four parents between whom I hardly distinguished in point of love and loyalty". His foster parents encouraged his interest in birds, snakes, insects, toads, and all living creatures even to the extent of setting aside a room for his treasures.

The Chicago fire occurred on October 7, 8, 9, 1871. His father had taken up residence there at the time, practicing law. A schoolmate had been engaged to drive the carriage to Chicago and he took young Wilson along. The fire was at its full height, but they had not heard of it at the time. Halted at the Chicago River they could see great clouds of black smoke. They spent the night on the floor of a house and in the morning took a roundabout course to the north side of "Phil's house". They found the place but Phil's father's house was gone. Still they identified it by a pile of Laraby furniture on the opposite side of the street, including a piano on which one of Phil's brothers was playing a lively waltz, while two others were dancing on the pavement. Later in the day they found Wilson's father's house on the South Side. The fire had not quite reached it.

In the following year when Wilson was not quite sixteen his uncle Davis suggested that he take over the "little country district school" that his brother Charles had taught the year before. The offer was thirty dollars a month and board (with his aunt and uncle). "At the end of the winter a farmer, the father of three of my pupils, said to my aunt, 'He's young but his age don't hurt him none'." "Few words of praise in my life have pleased me as much". "It was a grand experience that I would not have missed, as the saying is, for a farm. I lived at the manor house with my uncle and aunt, sleeping with one of the higher class men, during a very cold winter in the coldest room I ever entered, with no fire and often breaking the ice in the pitcher for the morning wash. The little one-room schoolhouse lay a mile away on the rolling prairie near the road leading to the village of Oswego. It was a pleasant but lonely country with the nearest house half a mile away. Every morning even in the dead of winter I walked to school carrying my luncheon in a tin pail, often with an icy wind blowing across the prairie, and had to build a fire in the schoolhouse and sweep the floor

before school opened at nine o'clock. When the thermometer stood at thirty degrees below zero, as it did at times, this was, I assure you, no joking matter. I wonder how the modern city-bred youth would like such an experience. I had only twenty-five pupils or so, of all ages from six to eighteen, and I had to teach all grades, from the three R's up to history and algebra."

His father urged him to take a competitive examination for West Point which he did and found extremely easy. He came out first on the list; "for it covered only ordinary grammar and school subjects", but since it was found that he was below the legal age, he was ruled out.

In the following summer he was in Geneva where his cousin, Sam Clarke, had just returned from Antioch College. "As the summer passed I had gradually made up my mind to try for a college education and a life devoted to biology or at least to science." "I had nothing but my two hundred dollars but with this in hand I packed up my meager outfit in September and started for Antioch College in southern Ohio." He rented a room for a dollar a week, joined an eating club which cost two and a half dollars a week, and earned a dollar a week by "manufacturing" the gas by which the college was lighted. The college was a very simple one but with sound ideals. "We had good teachers. Here, for the first time I received regular instruction in zoology and botany, in Latin, in geometry and trigonometry and especially in chemistry with regular laboratory work and I reveled in it all."

In June (1873) he went back to Geneva where with a tutor he began to study Greek. Instead of returning to Antioch in the fall a new prospect opened. Sam Clarke wrote enthusiastic letters from the Sheffield Scientific School at Yale. "In turning towards Yale I was influenced not merely by Clarke's example but in part by the reputation of the professors of zoology, botany, comparative anatomy, and geology, and in part by the almost equally compelling consideration to poor students that Yale offered many advantages in the way of self-support. I felt, however, not yet fully prepared—." Wilson spent the winter with his family in Chicago in attendance at the University there, preparatory to Yale. Hearing of an opening as recorder in the Lake Survey he easily passed the examination,

and was accepted at a salary of a hundred dollars a month. He took part in the primary triangulation of Lakes Ontario and Erie which lasted until September; then he started east to enter Yale.

He entered the Scientific School at Yale in 1875 and graduated three years later with the degree of Ph. B. He remained there one year more doing graduate work and acting as assistant. During the first year at Yale he took courses in zoology with Verrill, in botany with Eaton, and embryology with S. I. Smith. He then decided to regularize his work so that it would lead to the bachelor's degree. His three undergraduate years were, he writes, very busy and very happy years. It was during this year that he attended "Brewer's famous lectures, nominally on stock breeding but mainly on heredity and evolution. This was my first real approach to this subject. Brewer made an indelible impression on all who knew him, and especially on the youngsters who were fortunate enough to hear him. He lectured with the utmost fire and vehemence". At the end of the last year at Yale, Wilson was offered a position for the following year but both Sedgwick, with whom he was on intimate terms, and Wilson himself were getting roseate reports from Sam Clarke who was then at Johns Hopkins University. Both applied for fellowships there and were duly appointed. It was during the last year at Yale that Professor S. I. Smith showed Wilson a paper by Professor Mark of Harvard, saying "Here's a man who has written two hundred pages about the development of the snail and has only got as far as the 2-cell stage". "I wondered what the author could find to fill two hundred pages on that subject. I looked over the paper and saw my first picture of karyokinesis. Then and there was born my first determination to find out something about cells, protoplasm, cell division, fertilization, and development. And from that determination I have never swerved, although it often seems to me that cell structure and cell life seem in their essentials as mysterious today as they did fifty years ago".

Before leaving Yale, Wilson had been appointed a member of the U. S. Fish Commission (summer of 1877). The party under the leadership of Baird gathered at Gloucester, Massachusetts. Several trips were made on the naval steamer *Speed-*

*well*, where for the first time Wilson saw dredging for marine animals.

At the end of his first year at Johns Hopkins both he and Sedgwick were reappointed to fellowships and in the third year to assistantships. His three years there opened a new world of ideals; he became aware, he says, of new horizons of research, and wider outlooks in biology. His teachers were H. Newell Martin and W. K. Brooks. It was with the latter that he carried out most of his own work. "It was through informal talks and discussions in the laboratory, at his house, and later at the summer laboratories by the sea that I absorbed new ideas, new problems, points of view, etc." "Through him I first discovered what I really wanted to do." "From him I learned how closely biological problems are bound up with philosophical considerations. He taught me to read Aristotle, Bacon, Hume, Berkeley, Huxley; to think about the phenomena of life instead of merely trying to record and classify them."

Wilson had more and more wished to study in Germany but had no means to accomplish the trip. His eldest brother offered to help him with a loan and due to this brotherly generosity he was able to carry out his longfelt desire to study abroad. At the end of the summer of 1882 he sailed to Liverpool. Newell Martin had given him a letter to Huxley who expressed much interest in the work on *Renilla*. Later he arranged to have the memoir on *Renilla* published by the Royal Society.

Wilson settled down at Cambridge. Balfour had been killed in the Alps but his assistants and students were there, and Wilson recalls meeting Adam Sedgwick, Heape, Caldwell and Bateson. He also met Michael Foster and attended his lectures. He returned to London to give his paper before the Royal Society and then left for Germany. After spending a few weeks in the small village of Thurm to familiarize himself with spoken German he went to Leipzig. Here he worked in Leuckart's laboratory and also attended a few of Ludwig's lectures on physiology. He introduced the section-cutting method that Caldwell had invented in England and it created a sensation. In Leipzig he heard a great deal of the best music.

While at Johns Hopkins, Wilson had heard about the Zoological Station at Naples. He had wanted to go there for some time

but a table cost five hundred dollars and this he could not afford. At this time his cousin, Samuel Clarke, came to the rescue with the proposal that Williams College, where he was professor, would subscribe for a table and that Wilson, if he wished, might occupy it for the first year and Clarke himself the following year provided Wilson would agree to act as a substitute for him at Williams during his absence.

Naples produced a deep and lasting impression on Wilson. The Station came up fully to his expectations. There he came to know Anton Dohrn with whom he formed a sincere friendship, Hugo Eisinger, Edouard Meyer, and Arnold Lang—names that are familiar to many American zoologists who have followed in Wilson's footsteps to Naples. He has written: "That first year in Naples—it was not quite a year—was the most wonderful year of my life. I despair of conveying any notion of what it meant to me, and still means, as I look back upon it through the haze of fifty years. It was a rich combination of serious effort, new friendships, incomparable beauty of scenery, a strange and piquant civilization, a new and charming language, new vistas of scientific work opening before me; in short, a realization of my wildest, most unreal, dreams."

On his return from Naples he carried out the agreement to act as a substitute for Clarke at Williams College (1883-1884). While he gained some experience in teaching and made some pleasant new associations he was not sorry when the year came to an end. Amongst other duties he was expected to give a popular evening course on modern advances in biology, which interested him, he says, more than teaching the elementary classes. Darwin's theory of evolution, while widely accepted by scientists, was by no means acceptable to the evangelical public, including the president of the college. "On the whole," Wilson writes, "the year at Williams was scientifically a dead loss; I had no time nor appliances for research, no scientific stimulus, no incentive for research."

Sedgwick and Wilson had been much interested in the course in biology at Johns Hopkins given by Newell Martin, where in the laboratory the well-known book of Huxley and Martin was used. While at Williams they began planning along somewhat different lines a textbook of general biology. In part to carry

out their plan of collaboration Wilson was offered a lecture-ship with Sedgwick at the Massachusetts Institute of Technology. The book appeared in 1885 and was very successful.

Bryn Mawr College, a new institution for the higher education of women, was to be opened in the fall of 1885 and Wilson was invited to take charge of the department of biology. The college had been founded by Quakers and from the beginning adopted a liberal, even advanced, policy in its educational aims. This policy was largely due to Miss M. Carey Thomas who as Dean and later as President introduced the same standards as those followed by Johns Hopkins. Wilson taught at Bryn Mawr from 1885 to 1891 and had wonderful success, attracting to his classes many of the ablest students in the college.

Henry Fairfield Osborn had accepted a call from Columbia to establish a new department of zoology. He offered Wilson the position of adjunct professor to co-operate with him in organizing the new department. The offer included an arrangement by which Wilson would be given a year of foreign study before starting on his duties at Columbia. The second year in Europe was spent mainly in Munich and Naples and was even more productive and delightful than the first one, scientifically, because it settled definitely his later line of study, namely, cellular and experimental embryology. Boveri was at that time in Munich, and it was his presence there that had determined Wilson's choice of a place to work. Boveri was "far more than a brilliant scientific discoverer and teacher. He was a many-sided man, gifted in many directions, an excellent musician, a good amateur painter, and we found many points of contact far outside of the realm of science." "The best that he gave me was at the Café Heck where we used to dine together, drinking wonderful Bavarian beer, playing billiards, and talking endlessly about all manner of things."

At the end of the year he went to Naples with the Norwegian Hjort as travelling companion. At the Station he met Driesch and Herbst, both students of experimental embryology which at that time was a relatively new field and to which Wilson was soon to make valuable contributions. Driesch's work on the experimental production of twins interested Wilson intensely, because of its bearing on his own work on the development of

the earthworm and *Nereis*, then in press. In the spring of 1892 he went to Sicily to study the embryology of *Amphioxus*. Returning to Naples he sailed for Genoa where he saw the famous Joseph Guarnerius violin of Paganini. "The thrill that it gave me was only equalled by my ascent of Etna."

In 1904 Wilson married Anne Maynard Kidder, the daughter of Dr. Jerome Henry and Anne Maynard Kidder. Dr. Kidder was a friend of Spencer F. Baird who established the United States Fish Commission at Woods Hole. The Kidder family, who lived in Washington, D. C., built a summer cottage at Woods Hole and were on the most friendly terms with members of the Marine Biological Station. After the death of Dr. Kidder, Mrs. Kidder continued to go to Woods Hole. Many of us will remember her as charming, cultivated, witty, and hospitable and she was regarded by us as much a member of our group as though an official member of it. It was at Woods Hole that Wilson first knew Anne Kidder whose marriage to him added officially another valuable member of that family to the Woods Hole group. Their daughter, Nancy, Mrs. John Lobb, became a professional cellist of outstanding ability, and, during the latter years of Wilson's life, one of his greatest pleasures was watching her progress in her profession. This, in a sense, rounded off Wilson's passion for music.

In the summer of 1906 Wilson, accompanied by his wife, made an extensive collecting trip, first through the southeastern states, then to Knoxville, Tennessee, which was the early home of Mrs. Jerome Henry Kidder (the mother of Mrs. Wilson) and of her grandfather, Horace Maynard. From New Orleans they went to Tucson, Arizona, where they found in abundance the insects for which they were searching. The trip carried them as far as Wyoming and Southern California. This extensive journey was undertaken to collect insects in the study of whose chromosomes Wilson was then interested. He had taken part in the discovery of the role of the sex chromosomes in sex determination, and wished to extend the work over a wider field. The material collected served as a basis for some of the work carried out in following years at Columbia University.

It is difficult for one not himself a musician to describe Wil-

son's deep interest in music, and I shall follow his own words as far as possible. He has left a memorandum of his connection with musicians and music that shows how much music meant to him throughout his life. He writes, "I have always loved music and to it I owe some of the greatest pleasures of my life." His father played both the violin and cello, his mother played the piano and so did both of his aunts; his brother Charles was a good violinist, and his sister Ellen was an excellent pianist. There were many music lovers in Geneva and they formed, with a small group of professionals from Chicago, an orchestral organization.

His own musical experience began with singing lessons, but while he had no singing voice, as he says, he owes to these lessons "an inveterate habit of recalling all music in do-re-mi language." He learned to play the flute but later gave it up. It was in Baltimore that a new era in music opened for him when he attended the concerts of the Baltimore Conservatory of Music. He resolved never to touch the flute again and began to take lessons on the cello. He writes, "I was too old to take up so difficult an instrument with any hope of mastering it," but nevertheless he finally became an accomplished player in quartets. At Bryn Mawr he got in touch with a number of amateur musicians and had an immense amount of pleasure from these contacts. Later his first real introduction to quartet playing took place in Germantown at the house of Judge Penrose. After three or four years he became "a confirmed quartet lover of chamber music".

When he moved to New York he soon found a group of musical friends who formed a quartet that played for several years. Later when at Naples he was introduced through the invitation of Anton Dohn, himself "music mad," to some of the best musical society of Berlin. Here he came to know Joachim and the brothers Robert and Franz Mendelssohn who "between them owned a whole quartet of fine Stradivari fiddles". Needless to say Wilson had a wonderful time which he characterizes as a "musical debauch". "Music," Wilson writes, "has always seemed to me the most mysterious of the fine arts," "a language *sui generis* and one that often cannot really be translated into words."

Wilson's first extensive work "The Development of *Renilla*" was published in 1883 in the Philosophical Transactions of the Royal Society, London.\* It was a splendid piece of descriptive work, admirably presented. The sixteen plates that illustrate the text are an example of his skill and taste in drawing. It is worthy of note that by the use of serial sections, a procedure that was just coming into practice at that time, he was able to show in detail the cellular changes taking place in the interior of the opaque larvae of *Renilla*. The drawings of the sections also illustrate the excellence of his draftsmanship. The materials for the paper were collected at Beaufort, North Carolina, during two summers, when Wilson was a member of the Chesapeake Marine Station established by Johns Hopkins University.

During the six years he taught at Bryn Mawr College (1885-1891) he published a brief account of the movements of *Hydra*, and an extensive paper on the embryology of the earthworm (1889). After his appointment to Columbia University (1891) his research productivity steadily increased. In 1892 he published "The Cell-Lineage of *Nereis*". Dr. E. A. Andrews of Johns Hopkins was the first to discover the abundance of the night swimming *Nereis* at Woods Hole, and called attention to the splendid material it supplies for embryological study. Ever since that time it has furnished fine material to the embryologists working at the Marine Station there. Wilson's beautiful paper on the development of *Nereis* may be said, in America at least, to have inaugurated a long line of research on cell lineage.

It is interesting to look back and ask what motivated these enormously laborious studies on cell lineage. They followed closely on the heels of the descriptive period of embryology whose leading idea was that the history of the race was repeated in the development of the individual. The current phrase was Ontogeny repeats Phylogeny, a speculation that had reached its climax in the exaggeration of Ernest Haeckel. As facts accumulated skepticism increased as to the validity of this postulate, especially when different observers drew different conclusions

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\* Wilson had earlier (1879-1881) published two systematic papers on Pycnogonida or sea spiders while at Yale. These served as his graduate thesis for the degree of Ph. B.

from the same facts, particularly when the interrelations of the larger groups were concerned. Nevertheless certain evidence could not be ignored, such, for example, as the presence of gill slits in the early stages of the bird and mammal which appeared to repeat the gill system of the adult fish. However, when attention was called to the fact that gill slits, similar to those of the bird and mammal, were present in a corresponding early stage of the fish it became evident that it was not the adult gill system of the fish that was repeated in the higher form, but rather the retention of the same stage of development as that in the embryo fish. This conclusion seemed more consistent than the older somewhat mystical idea of adult stages of the lower forms being telescoped back into the early stages of the higher forms. Furthermore, the theory of the repetition of similar embryonic structures could be used to support the evolution theory just as well as the older phylogenetic theory. It furnishes as good evidence as that from comparative anatomy, but goes even further in offering earlier clues as to relationships between the great groups of the animal kingdom.

Wilson was not much concerned with the ontogeny-phylogeny theory, although in his earlier paper on *Renilla* he had followed the spirit of the times in his discussion of the gastraea theory as a recapitulation of a two-layered ancestor of the higher animals. He was, however, more concerned with comparisons between the cleavage stages of annelids, molluscs, and flatworms (1895-98), and still more concerned with phenomena of cleavage in these forms as having a bearing on what was called the "organization of the egg".

The problem of the organization of the egg was an old one, but after the experimental work of Roux on frogs' eggs and that of Chabry on ascidians' eggs, and the experimental work of Driesch on sea urchins' eggs, the theoretical deductions that they drew from these experiments, which were opposed, aroused wider and wider interest. From that time onwards the older phylogenetic problems lost interest, and embryologists took up the experimental study of embryology with increasing success and enthusiasm. Much of Wilson's later work was concerned with the evidence and its discussion in this new field.

Wilson's first piece of work in experimental embryology was

carried out at the Faro, Sicily, in 1892 and published in 1893. The Faro was the original hunting ground of Kowalevski and Hatchek, where they obtained their material for their splendid papers (1867 and 1881). Wilson found great variability in the early cleavage stages of *Amphioxus*, but it is not quite clear to what extent they are due to abnormal conditions. In the light of the later work of Conklin (1932, 1933) it appears that, in the redistribution of the protoplasm of the egg of *Amphioxus* preceding the early stages of cleavage, the egg follows the mosaic type about as closely as does the Ascidian egg. Wilson found that the isolated first blastomeres produce whole embryos, as do the first four blastomeres also, and even the  $\frac{1}{8}$  blastomere. More recent work (Conklin 1933) confirms Wilson's conclusion for the two-cell stage, but it seems probable that only certain isolated blastomeres of the four-cell stage are capable of giving whole embryos.

Wilson's second work in experimental embryology (1901) dealt with the effects of agents inciting artificial parthenogenesis of the eggs of the sea urchin. The cytasters that appear after treatment with various salts had already been described in some detail by me (1896, 1899, 1900), but Loeb had been more successful in rearing embryos from eggs treated with magnesium chloride, and Wilson, by use of Loeb's procedure, described in more detail the role of the artificially induced cytasters. In the same year he published results that dealt with the suppression of the incipient cleavage plane by treatment with ether. He followed especially the disappearance of the asters and their reappearance when the eggs were returned to sea water, and drew certain conclusions as to their nature. In the same year he described results that followed shaking the eggs of the sea urchin just at the beginning of the first division. "The cleavage furrow, which may have begun to cut into the egg, is thus caused to fade out more or less completely and a binucleate condition results closely similar to that seen in the etherized eggs, but in most cases differing from the latter distinctly in that the elongation is still retained and its general outline at this time is very nearly identical with that of the 2-cell stage in which the cleavage furrow is obliterated. In such eggs the second cleavage takes place as a rule in the manner described by Boveri, two

parallel amphiasters being formed through the equatorial plane of which the cleavage furrow passes exactly as though the first division had been completed. Two binucleated blastomeres are thus formed, and in typical examples this process is repeated at the third cleavage to form four binucleate blastomeres". Boveri had previously studied the same problem (1897), suppressing the first cleavage by placing the egg under pressure. "My observations", Wilson writes, "differ from Boveri's, however, in showing very clearly that after this point had been reached the first cleavage is restored, at least in some of the cells, and it may be restored even earlier".

In 1902 a graduate student, W. S. Sutton, pointed out that the two maturation divisions furnish an explanation of Mendel's laws. Wilson writes, "during the past year working in my laboratory he has obtained more definite evidence in favor of this result (the separation of maternal and paternal chromosomes), suggested by Montgomery (1901), which led him to the conclusion that it probably gives the explanation of the Mendelian problem". This conclusion of Sutton's has turned out to be more than "probable", and is today the basis for the mechanism of Mendel's two laws.

At Beaufort in the summer of 1903 Wilson repeated on *Alpheus* the experiments that Przibram had made in 1901 on the reversal of the large and small claw of *Alpheus* after removal of the smaller one, and confirmed Przibram's conclusions.

In the summer of 1902 Wilson made experiments with the nemertine egg, that Coe had previously shown to offer "an almost ideally perfect illustration of this type (mosaic) of cleavage". The material was studied at the marine laboratory of South Harpswell, Maine. Wilson studied both egg-fragments and isolated blastomeres. He concluded that localization of regions of the egg is a progressive (epigenetic) process. Before ripening the germinal regions of the egg of Nemerteans are equipotential (since fragments from such eggs may give rise to normal embryos) as regards the factors for cleavage and localization. The factors are to a certain extent localized between the beginning of ripening (the extrusion of the polar bodies) and the completion of the first cleavage; nevertheless, owing to the property of regulation a complete embryo develops out of an

isolated blastomere. The localization process takes place through a new distribution of cytoplasmic materials. Cleavage is a means of localizing this material and is not the cause of differentiation. Here Wilson faced an apparent contradiction between whole and part development—a problem that is today still taxing the ingenuity of experimental embryologists. Wilson's own experiments showed that certain fragments of the *Cerebratulus* egg, that have reached the blastula stage, may still give rise to complete or nearly complete larvae. Also while the isolated blastomere cleaves, as though still a part only of the whole, it gives rise to a perfect embryo. Other and later experiments by Yatsu, Zeleny and Hörstadius have added further information concerning the potencies of parts of the egg and isolated blastomeres of *Cerebratulus*.

The most complete and outstanding papers that Wilson published in 1904 deal with "Experimental Studies on Germinal Localization". The first deals with the egg of *Dentalium*; the second with *Patella* and *Dentalium*. These papers were the outcome of eight months' residence at the Naples Zoological Station in 1903. The egg of the mollusc *Dentalium* has three distinct zones that can be traced to definite parts of the segmented egg. The first two blastomeres are unequal in size. If the smaller is isolated the embryo lacks certain organs; if the larger is isolated it gives rise more nearly to a whole embryo. A yolk lobe normally appears on one of the first two blastomeres. If removed the blastomere develops into a larva lacking certain organs present in the normal embryo. The unfertilized egg was also cut into fragments and these were fertilized. If cut in two in a horizontal plane, Wilson writes:—

"The upper fragment segments symmetrically without the formation of polar lobes and produces a larva similar to the lobeless ones. The lower one segments like a whole egg of diminished size, and may produce a normally formed dwarf trochophore. Fragments obtained by vertical section through the lower white area may segment like whole eggs and may produce nearly normally formed dwarf trochophores. Enucleated fragments, containing the lower white area of fertilized eggs, pass through alternating periods of activity and quiescence corresponding with the division-rhythm of the nucleated half, and form the polar lobes as if still forming part of a complete em-

bryo. The same is true of the isolated polar lobe. The foregoing observations demonstrate the prelocalization of specific cytoplasmic stuffs in the unsegmented egg and their isolation in the early blastomeres. The lower white area contains such stuffs as are essential to the formation of the apical organ and the complex of structures forming the post-trochal region, including the shell-gland and shell, the foot, the mantle-folds and probably the coelomesoblast. These stuffs are contained in the first polar lobe, but the second lobe no longer contains those necessary for the basis of the apical organ. Progressive changes therefore occur in the original distribution of the specific cytoplasmic materials. Comparison indicates that the conditions observed in the molluscan egg differ only in degree from those in the nemertine or echinoderm. These differences reduce themselves to differences in the period of segregation (or differentiation) and in its pattern, and are explicable under the general theory of precocious segregation. The early development of egg-fragments indicates that the specification of the cytoplasmic regions is primarily qualitative, but not quantitative, or if quantitative is still subject to a regulative process that lies behind the original topographical grouping of the egg-materials. The development of the molluscan egg is in its essential features a mosaic-work and sustains the theory of 'Organbildende Keimbezirke'".

The second paper deals largely with another mollusc, *Patella*, and describes the development of isolated blastomeres. The results are the same as those on *Dentalium*, but are carried further including the differentiation of later isolated blastomeres. The conclusion is reached that "the development of both *Patella* and *Dentalium* is essentially a mosaic work of self-differentiating cells".

In the same year Wilson stated his general conclusions as follows:

"I would express the opinion that, so far as the early stages of development are concerned, it is difficult to escape the hypothesis of formative stuffs or specific morphoplasmic substances, in some form. But while this hypothesis facilitates an understanding of the *modus operandi* or immediate causes of differentiation, it leaves us as much as ever in the dark as to the localizing or form-determining factors which are responsible for the determination of the segregation pattern. This problem, which is essentially one of correlative action, is not only unsolved, but suggests the existence of specific energies for

which it is difficult at present to find an analogy outside the field of protoplasmic action”.

A year later (1905) in a public address Wilson posed the two outstanding problems of development, as follows:

“I need not dwell on the absorbing, almost tantalizing, interest with which the problem of development has held the attention of naturalists from the earliest times. Twenty centuries and more have passed since Aristotle first endeavored to trace something like a rough outline of its solution. The enormous advances of our knowledge during this long period have taken away nothing of the interest or freshness of the problem; they have left it, indeed, hardly less mysterious than when the father of science wrote the first treatise on generation. I will not dwell on the epoch-making work of Harvey, Wolff and von Baer, or the curious, almost grotesque controversies of the eighteenth century, when embryology invaded the field of philosophy and even of theology. I will only point out that even at that time, when embryology was almost wholly limited to the study of the hen’s egg, embryologists were already occupied with two fundamental questions, which still remain in their essence without adequate answer, and though metamorphosed by the refinements of more modern observation and experiment still stand in the foreground of scientific discussion. The first of these is the question of preformation versus epigenesis—whether the embryo exists preformed or predelineated in the egg from the beginning or whether it is formed anew, step by step in each generation. The second question is that of mechanism versus vitalism—whether development is capable of a mechanical or physico-chemical explanation, or whether it involves specific vital factors that are without analogy in the non-living world”.

On the preformation view:

“Development was conceived to be only the unfolding and transformation of a pre-existing structure, not the successive formation of new parts—a process of ‘evolution’, not of epigenesis. In this particular form the doctrine of preformation was conclusively overthrown by Wolff; but the principle underlying it has repeatedly and persistently reappeared in later speculations on development, and still contests the field of discussion with its early antagonist”.

With respect to the second question Wilson wrote:

“Hand in hand with this controversy has gone one of still more general scope between the two opposing conceptions that

I have referred to as mechanism and vitalism. Is development at bottom a mechanical process? Is the egg a kind of complex machine, wound up like a piece of clockwork, and does development go forward like the action of an automaton, an inevitable consequence of its mode of construction? Or, on the other hand, does development involve the operation of specific vital entelechies or powers that are without analogue in the automaton and are not inherent in any primary material configuration of the egg? This question, I hardly need say, is included in the larger one, whether the vital processes as a whole are or are not capable of mechanical explanation. As a problem of embryology it is very closely connected with that of preformation or epigenesis, and in point of fact the two have always been closely associated. Evidently, by its very form of statement, any theory of preformation or prelocalization in the germ assumes at least a mechanical basis for development, i.e., a primary material configuration upon which the form of development in some measure depends. With theories of epigenesis the case is not so clear; for such theories may or may not be mechanical. Without further preamble I now ask your attention to certain facts which will place clearly before us the form in which these time-honored problems appear to us today".

These quotations serve to show the philosophical morass in which the embryologists were floundering at that time. The older dispute between preformation and epigenesis has been largely laid aside in its original meaning, but to some extent has been brought forward under the evasive term of "organization of the egg". Today vitalism, as propounded by Driesch, has almost been forgotten, as embryological research has advanced, but in another form called the "Organism as a Whole" or the organismal theory it still has advocates and serves to keep the philosophical ball rolling—but to what goal is as unclear as it was forty years ago.

Wilson's most outstanding contributions are his eight studies on chromosomes published from 1905 to 1912. These deal almost exclusively with the reduction divisions during spermatogenesis. Here accuracy of observation and care in interpretation of the behavior of the chromosomes are shown in a high degree. The actual counts of the number of chromosomes is in itself not difficult, at least in those forms that have a small number, and Wilson chose mainly such forms, but the changes that take place during the ripening of the sperm cells call not

only for extraordinarily careful observations but also for skill in interpretation. In both respects Wilson was unusually gifted. None of his results has been rejected by later workers while some of the erroneous chromosome counts of other contemporary cytologists held back for several years the solution of the role of the sex chromosomes in the determination of male and female.

In 1891 Henking described in the sperm cells of an insect *Pyrrochoris* a body that he called a chromosome nucleolus that finally went to half the sperm cells. But he did not even suggest that it had anything to do with sex determination. In 1902 McClung described a similar body in grasshoppers and called it the accessory; later it was called the X-chromosome. He suggested further that since half the sperm came to contain it and half lacked it, it had to do with sex, the male having one more chromosome than the female. Unfortunately the reverse turned out to be true in the insects under consideration. It is surprising that between 1902, when McClung published his hypothesis, and 1905, no one tested this view by an examination of the chromosomes of females. The reason is that it is difficult to procure good material of the maturation stage in the female while the corresponding stages in the male are readily procurable and give beautifully clear preparations. It is true there were a few records of oögonial chromosomes in females, but it turned out that the counts were wrong or else the male count was wrong, which stood in the way of acceptance or rejection of McClung's hypothesis. Then in 1905 Miss Nettie Stevens at Bryn Mawr College published in the Publications of the Carnegie Institution of Washington an account of the role of the sex chromosomes in the beetle *Tenebrio*. She showed that the male had 19 large and 1 small chromosome (the Y), the latter going to half the spermatozoa. She also showed that at the reduction division it (the smaller one) was the mate of one of the large chromosomes. Consequently half the ripe sperm had 10 large chromosomes and half had 9 large and 1 small chromosome. In the oögonial cells there were 20 large chromosomes which would reduce to 10 in the egg after maturation. She pointed out that an egg fertilized by a sperm with 10 large chromosomes would give a female with 20 such chromosomes,

and that an egg with 10 large chromosomes fertilized by a sperm with 9 large and 1 small chromosome would restore the number characteristic of the male.

In the same year (1905)<sup>1</sup> Wilson published a similar conclusion in regard to the role of the sex chromosomes in two other insects in which the female has one more chromosome than the male; thus *Anasa tristis* ♀ has 22, and the male 21; and *Protenor* ♀ has 14, and the ♂ 13. The Stevens type XX-XY and the Wilson type XX-XO are the same in principle. It has turned out that the former is much commoner than the latter as a sex determining mechanism occurring widely in groups other than insects.

Another mechanism was discovered later in birds and moths in which the male was homozygotic for the sex chromosomes,

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<sup>1</sup>The question is sometimes asked as to the priority of Stevens' and Wilson's papers. Stevens' paper was handed in on May 15, 1905, and printed in September of that year. In Wilson's paper "Studies on Chromosomes" I (dated May 5, 1905; published August 1905) he says in a footnote: "The discovery, referred to in a preceding footnote, that the spermatogonial number of *Anasa* is 21 instead of 22, again goes far to set aside the difficulties here urged. Since this paper was sent to press I have also learned that Dr. N. M. Stevens (by whose kind permission I am able to refer to her results) has independently discovered in a beetle, *Tenebrio*, a pair of unequal chromosomes that are somewhat similar to the idiochromosomes in Hemiptera and undergo a corresponding distribution to the spermatozoa. She was able to determine, further, the significant fact that the small chromosome is present in the somatic cells of the male only, while in those of the female it is represented by a larger chromosome. These very interesting discoveries, now in course of publication, afford, I think, a strong support to the suggestion made above; and when considered in connection with the comparison I have drawn between the idiochromosomes and the accessory show that McClung's hypothesis may, in the end, prove to be well founded."

In Wilson's "Studies on Chromosomes" II (dated October 4, 1905; published November, 1905) he says, "During the summer, and since the foregoing paper was entirely completed in its present form, I have obtained new material which shows decisively that the theoretic expectation in regard to the relations of the nuclei in the two sexes, stated at p. 539, is realized in the facts. In *Anasa*, precisely in accordance with the expectation, the oögonial divisions show with great clearness one more chromosome than the spermatogonial, namely, twenty-two instead of twenty-one; and the same number occurs in the divisions of the ovarian follicle-cells. Again in accordance with the expectation, the oögonial groups show four large chromosomes instead of the three that are present in the spermatogonial groups. In other respects the male and female groups are closely similar. In like manner, the oögonial divisions in *Alydus* and *Protenor* show fourteen chromosomes, the spermatogonial but thirteen; and in *Protenor* the spermatogonial chromosome-groups have but one large chromosome (unquestionably the heterotropic) while the oögonial groups have two such chromosomes of equal size."

here called ZZ, and the female heterozygotic, or WZ. The mechanism is the same in principle here as in the other cases.

In 1929 and 1930 Wilson returned to the problems of experimental embryology. Centrifuging the unfertilized eggs of *Chaetopterus* breaks them up into fragments. These fragments may contain any part of the materials stratified by the centrifuge, nevertheless if fertilized many of them with different inclusions may develop into larvae "closely similar to the normal whole trochophores." The cleavage of the normal egg is typically determinate, and many of the fragments, even very small ones, may show the same type of cleavage as the whole egg. Of course, only relatively few of the fragments cleave or develop normally. Whether the eggs orient on the machine was at the time undecided, but in his second paper Wilson considered the question and concluded that the eggs orient themselves on the centrifuge to a large extent.

In 1916 and again in 1925 Wilson described the remarkable changes that take place during spermatogenesis in the chondriosomes of the scorpion, and in 1937 in collaboration with Pollister a more detailed study, including the Golgi bodies.

Five years after his appointment at Columbia University he published his book on "The Cell" (1896) which was at once recognized as the outstanding summary of the work in this field. Wilson drew upon his wide experience covering, as it did, the role of the cell in fertilization and development, in experimental embryology, in spermatogenesis as well as a thorough familiarity of the work of his contemporaries dealing with the cell. A third and greatly extended edition appeared in 1925. During the interval between the first and third editions, work in cytology had advanced in many directions and a voluminous literature had grown up. In a masterly way Wilson summarized this literature, separating the wheat from the chaff. I can not do better than quote here the words of Professor E. G. Conklin spoken at the time of the award to Wilson of the Daniel Giraud Elliot medal (for 1925) by the National Academy,

"The third edition of 'The Cell in Development and Heredity' has been written out of this unique experience; it represents not only the mature point of view of the world's leading student

and teacher of cytology, but it is to a large extent the work of its leading investigator in this field. Few other workers are left who were in at the birth of this science and who can speak of its development with the knowledge that comes from intimate contact with persons and problems, and no one could deal with this subject in a more comprehensive and judicial manner. Though called a third edition of the earlier work, this is in reality an entirely new book, rewritten from cover to cover and almost three times as large as the previous edition. It is in every respect a monumental work, one of the most complete and perfect that American science has produced in any field, and while we congratulate Professor Wilson upon this consummation of the work of a lifetime, we are proud of the fact that the National Academy of Sciences can bestow the Elliot Medal on a fellow member for a book of such outstanding worth as 'The Cell in Development and Heredity.' "

Wilson was a member of all the leading learned societies of Europe and America. He was a recipient of honorary degrees from the universities of Columbia, Harvard, Yale, Johns Hopkins, Chicago, Louvain, Cambridge (England), Lwow, and Leipzig. He was awarded the gold medal of the Linnean Society, London; the Elliot Medal of the National Academy of Sciences; the John J. Carty Medal and Award. He will be lovingly remembered by his many friends as a reserved, cultured gentleman whose sincerity, judgment, and breadth of knowledge were shown by the perfection of his lectures and his scientific papers.

PUBLICATIONS OF EDMUND BEECHER WILSON

1878-1937

1878

- The Pycnogonida of New England and adjacent waters. U. S. Commissioner's Report, Pt. 6.  
 Descriptions of two new genera of Pycnogonida. Amer. Jour. Sci. & Arts. 15: 1-4.  
 A synopsis of the Pycnogonida of New England. Trans. Conn. Acad. Arts & Sci., 5: 1-26.

1880

- Notes on the early stages of some polychaetous annelids. Amer. Jour. Sci., 20: 291-292.  
 The development of Phoronis: A contribution to the study of animal metamorphosis (abstract). Johns Hopkins Univ. Cir., Dec.: 82.  
 A problem of morphology as illustrated by the development of the earthworm (abstract). Johns Hopkins Univ. Cir., May: 66.  
 Preliminary abstract of observations upon the early stages of some polychaetous annelids. Zool. Anz., 3: 455-456.

1881

- Report on the Pycnogonida. Reports on the results of dredging, under the supervision of A. Agassiz, along the east coast of the United States during the summer of 1880, by the U. S. Coast Survey steamer "Blake", Commander J. R. Bartlett, U. S. N., commanding. Bull. Mus. Comp. Zool., 8.  
 The origin and significance of the metamorphosis of Actinotrocha. Quart. Jour. Micr. Sci., 21.

1882

- Observations on the developmental stages of some polychaetous Annelids. Stud. Biol. Lab., J. H. U.  
 On a new form of Pilidium. Stud. Biol. Lab., J. H. U. 2.  
 On animal polymorphism (abstract). Johns Hopkins Univ. Cir., May: 203.  
 Observations upon the structure and development of Renilla and Leptogorgia (abstract). Johns Hopkins Univ. Cir., Aug.: 247.  
 The development of Renilla (abstract). Proc. Royal Soc., No. 222.  
 Review of certain points in Murphy's "habit and intelligence." Johns Hopkins Univ. Cir.  
 Variation in the yolk-cleavage of Renilla. (With H. L. Osborn and J. M. Wilson.) Zool. Anz., 5: 4 pp.

1883

- The first zoëa of Porcellana. (With W. K. Brooks.) Stud. Johns Hopkins Univ.  
 The development of Renilla. Phil. Trans., 174.

1884

Mesenterial filaments of the Alcyonaria. *Mitt. Zool. Sta., Naples*, 5.

1886

General Biology. (With W. T. Sedgwick.) 193 pp. New York.

1887

The germ bands of *Lumbricus*. *Jour. Morph.*, 1.

Origin of the excretory system in the earth-worm. *Proc. Acad. Nat. Sci.*, 39: 49-50.

1888

Note on the mechanism of the thread-cells in *Hydra*. *Am. Mo. Micr. Jour.*, 9.

1889

The embryology of the earth-worm. *Jour. Morph.*, 3: 387-462.

1890

The origin of the mesoblast-bands in Annelids. *Jour. Morph.*, 4: 205-219.  
Some problems of annelid morphology. *Biol. Lect., Woods Hole*, 53.

1891

The heliotropism of *Hydra*. *Amer. Nat.*, 2: 413-433.

1892

The cell-lineage of *Nereis*. A contribution to the cytogeny of the annelid body. *Jour. Morph.*, 6: 361-480.

On multiple and partial development in *Amphioxus*. *Anat. Anz.*, 7: 732-740.

1893

*Amphioxus* and the mosaic theory of development. *Jour. Morph.*, 8: 579-638.

1894

The mosaic theory of development. *Biol. Lect., Woods Hole*, 2: 1893.

1895

The embryological criterion of homology. *Biol. Lect., Woods Hole*, 1894.  
Maturation, fertilization, and polarity in the echinoderm egg. New light on the "Quadrille of the Centers." (With A. P. Mathews.) *Jour. Morph.*, 10: 319-342.

Archoplasm, centrosome and chromatin in the sea-urchin egg. *Jour. Morph.*, 11: 443-478.

The quadrille of the centrosomes in the echinoderm egg; a recent contribution to biological mythology (abstract). *Science*, 1: 68.

Polarity of the egg in *Toxopneustes* (abstract). *Science*, 1: 68.

An atlas of fertilization and karyokinesis of the ovum. (With E. Leaming.) *Columbia Univ. Press*, New York.

1896

- The cell in development and inheritance. *Sci. Prog.*  
 The cell in development and inheritance. 371 pp. New York.  
 On cleavage and mosaic-work. *Arch. Entw.-mech.*, 3: 19-26.

1897

- Van Beneden and the origin of the centrosome. A correction. *Science*, 5: 25.  
 Centrosome and middle-piece in the fertilization of the egg. *Science*, 5: 114.

1898

- Considerations on cell-lineage and ancestral reminiscence, based on a re-examination of some points in the early development of Annelids and Polyclades. *Ann. N. Y. Acad. Sci.*, 11: 1-27.  
 Bradney B. Griffin. *Science*, n.s., 7.  
 New York Academy of Sciences, Biological Section. *Anat. Anz.*, 14: 350.

1899

- The structure of protoplasm. *Biol. Lect.*, Woods Hole, 1st, 1897, 1-20.  
 Cell lineage and ancestral reminiscence. *Biol. Lect.*, Woods Hole, 2nd, 1898, 21-42.  
 On protoplasmic structure in the eggs of Echinoderms and some other animals. *Jour. Morph.*, 15 (suppl.): 1-28.

1900

- The cell in development and inheritance. 2nd ed., 483 pp. New York.  
 Some aspects of recent biological research. *Intern. Mo.*, July, 1-22.  
 The chemical fertilization of the sea urchin. *Science*, 13.

1901

- Experimental studies in cytology. I. Cytological study of artificial parthenogenesis in sea-urchin eggs. *Arch. Entw.-mech.*, 12: 529-596.  
 II. Some phenomena of fertilization and cell-division in etherized eggs.  
 III. The effect on cleavage of artificial obliteration of the first cleavage furrow. *Arch. Entw.-mech.*, 13: 353-395.  
 The morphological phenomena involved in the chemical production of parthenogenesis in sea urchins. (Abstract.) *Biol. Bull.*, 2: 347-350.  
 Studien über den Körperbau der Anneliden. V. By E. Meyer. (Review.) *Science*, n. s., 14.  
 Aims and methods of study in natural history. (Presidential address, American Naturalist Society, Dec. 28, 1900.) *Science*, 13: 14-23.

1902

- Cell homology. (Report American Morphological Society.) *Science*, 15: 379.  
 Mendel's principles of heredity and the maturation of the germ-cells. *Science*, 16: 991-993.

1903

- Notes on the reversal of asymmetry in the regeneration of the chelae in *Alpheus heterochelis*. *Biol. Bull.*, 4: 197-210.  
 Notes on merogony and regeneration in *Renilla*. *Biol. Bull.*, 4: 215-226.  
 Experiments on cleavage and localization in the nemertine egg. *Arch. Entw.-mech.*, 16: 411-460.  
 Mr. Cook on evolution, cytology and Mendel's laws. *Pop. Sci. Mo.*, November: 188-189.

1904

- Mosaic development in the annelid egg. *Science*, 20: 748-750.  
 Experimental studies on germinal localization. I. The germ-regions in the egg of *Dentalium*. *Jour. Exp. Zool.*, 1: 1-72. II. Experiments on the cleavage mosaic in *Patella* and *Dentalium*. *Ibid.*: 197-268.

1905

- Cytasters and centrosomes in artificial parthenogenesis. *Zool. Anz.*, 28.  
 The problem of development. (Annual address of President, New York Academy of Sciences, Dec. 19, 1904.) *Science*, 21: 281-294.  
 Observations on the chromosomes in Hemiptera. (Report New York Academy of Sciences. May 8, 1905.) *Science*, 21: 548.  
 The chromosomes in relation to the determination of sex in insects. *Science*, 22: 500-502.  
 Studies on chromosomes. I. The behavior of the idiochromosomes in Hemiptera. *Jour. Exp. Zool.*, 2: 371-405.  
 Studies on chromosomes. II. The paired microchromosomes, idiochromosomes and heterotropic chromosomes in Hemiptera. *Ibid.*: 507-545.

1906

- Mendelian inheritance and the purity of the gametes. *Science*, 23: 112-113.  
 Studies on chromosomes. III. The sexual differences of the chromosome-groups in Hemiptera, with some considerations on the determination and inheritance of sex. *Jour. Exp. Zool.*, 3: 1-40.  
 A new theory of sex-production. *Science*, n. s., 23.

1906-07

- Recent studies of heredity. *Harvey Lectures*, 200.

1907

- The biological significance and control of sex. Sex determination in relation to fertilization and parthenogenesis. *Science*, n. s., 25.  
 The case of *Anasa tristis*. *Science*, 25: 191-193.  
 The supernumerary chromosomes of Hemiptera. *Science*, 26: 870-871.  
 Note on the chromosome groups of *Metapodius* and *Banasa*. *Biol. Bull.*, 12: 303-313.

1908

The accessory chromosome of *Anasa tristis*. (Read before the American Society of Zoologists, December, 1907). *Science*, 27: 690.  
Biology. Columbia Univ.

1909

Teaching and research of the natural sciences. Columbia Univ. Quart., March.  
Studies on chromosomes. IV. The "accessory" chromosome in *Syromastes* and *Pyrrochoris* with a comparative review of the types of sexual differences of the chromosome groups. *Jour. Exp. Zool.* 6: 69-99.  
Studies on chromosomes. V. The chromosomes of *Metapodius*, a contribution to the hypothesis of the genetic continuity of chromosomes. *Ibid.*: 147-205.  
Differences in the chromosome-groups of closely related species and varieties, and their possible bearing on the "physiological species". *Proc. 7th Intern. Zool. Congr.* 1-2.  
Photographic illustrations of the morphological and physiological individuality of the chromosomes in Hemiptera, with an account of observations on the chromosomes in the living cells, (abstract). *Proc. 7th Intern. Zool. Congr.*, 1-3.  
The supernumerary chromosomes and their relation to the "odd" or "accessory" chromosome. *Proc. 7th Intern. Zool. Congr.*  
The sexual differences of the chromosome groups in *Pyrrochoris* and *Syromastes*. *Science* 29: 426.  
The female chromosome groups in *Syromastes* and *Pyrrochoris*. *Biol. Bull.* 16: 199-204.  
Secondary chromosome-couplings and the sexual relations in *Abraxas*. *Science* 29: 704-706.  
Recent researches on the determination and heredity of sex. (Vice-Presidential address, American Association for the Advancement of Science, Baltimore, 1908). *Science* 29: 53-70.  
The cell in relation to heredity and evolution: in "Fifty years of Darwinism". New York.

1910

Note on the chromosomes of *Nezara*. *Science* 31: 803.  
Studies on chromosomes. VI. A new type of chromosome combination in *Metapodius*. *Jour. Exp. Zool.* 9: 53-78.  
The chromosomes in relation to the determination of sex. *Sci. Prog.* 16: 570-592.  
Selective fertilization and the relation of the chromosomes to sex-production. *Science* 32: 816.

1911

The sex chromosomes. *Arch. mik. Anat.*, 77: 249-271.  
Studies on chromosomes. VII. A review of the chromosomes of *Nezara*; with some more general considerations. *Jour. Morph.*, 22: 71-110.

EDMUND BEECHER WILSON—MORGAN

The matter of university fellowships. Letter to the editor of "Science".  
*Science* 33: 33.

The memorial to Anton Dohrn. *Science* 34.

1912

Studies on chromosomes. VIII. Observations on the maturation-phenomena in certain Hemiptera and other forms, with considerations on synapsis and reduction. *Jour. Exp. Zool.*, 13: 345-448.

Some aspects of cytology in relation to the study of genetics. (Read before American Society of Naturalists, Princeton, Dec. 1911.) *Amer. Nat.*, 46: 57-67.

Observations on synapsis and reduction. *Science*, 35: 470-471.

Thomas Harrison Montgomery. *Arch. Zellf.* 9.

1913

A chromatoid body simulating an accessory chromosome in *Pentatoma*.  
*Biol. Bull.*, 24: 392-404.

Heredity and microscopical research. *Science*, 37: 814-826.

1914

The bearing of cytological research on heredity. (Croonian Lecture, delivered June 11, 1914.) *Proc. Royal Soc.* 88: 333-352.

1915

Science and liberal education. *Science*, 42: 625-630.

Some aspects of progress in modern zoology. *Smithsonian Rept.*

1916

The distribution of the chondriosomes to the spermatozoa in scorpions.  
*Proc. Nat. Acad. Sci.*, 2: 321-324.

1920

Chiasmatype and crossing over. (With T. H. Morgan.) *Amer. Nat.*, 54: 193-219.

1922

Wm. T. Sedgwick (1855-1921). *Proc. Amer. Acad. Arts & Sci.*, 57: 512-516.

1923

The physical basis of life. (1st Sedgwick Memorial Lecture, delivered in Boston, Dec. 29, 1922.) *Science*, 57: 277-286.

1924

Reorganization of the Naples Zoological Station. *Science*, 59: 182-183.

1925

Protoplasmic systems and genetic continuity. *Amer. Nat.*, 59: 481-496

The cell in development and heredity. 3rd ed., 1232 pp. New York.

The sex chromosomes of sea-urchins. *Science*, 61.

Extract from address on the Marine Biological Laboratory. *Science*, 62: 275.

1926

The Naples Zoological Station. *Science*, 63.

Newer aspects of the alveolar structure of protoplasm. *Amer. Nat.*, 60: 105-120.

1928

The production of dwarf larvae from fragments of the annelid egg and the problem of localization. *Science*, 68.

The cell in development and heredity. 3rd ed. with corrections. New York.

1929

The development of egg-fragments in Annelids. *Arch. Entw.-mech.* (Festschrift für Hans Spemann), 117: 179-210.

1930

Notes on the development of fragments of the fertilized Chaetopterus egg. *Biol. Bull.*, 59: 71-80.

The question of the central bodies. *Science*, 71: 661-662.

1931

The central bodies again. (With A. F. Huettner.) *Science*, 73: 447-448.

The distribution of the sperm-forming materials in scorpions. *Jour. Morph.*, 52: 429-483.

Biology. 1904-1929. Quart. Cent. Learning, Columbia Univ. Press.

1932

Polyploidy and metaphase patterns. *Jour. Morph.*, 53: 443-471.

1937

Observations on sperm formation in the centrurid scorpions with especial reference to the Golgi material. (With A. W. Pollister.) *Jour. Morph.*, 60: 407-443.





*Edw L Nichols*

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BIOGRAPHICAL MEMOIRS

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BIOGRAPHICAL MEMOIR

OF

EDWARD LEAMINGTON NICHOLS

1854–1937

BY

ERNEST MERRITT

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PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1940

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## EDWARD LEAMINGTON NICHOLS

1854-1937

BY ERNEST MERRITT

It was not until nearly the close of the last century that physics became firmly established in America as one of the sciences to which this country is every year making significant contributions. Previous to the year 1890 a few physicists were doing work of outstanding importance; but in the main the departments of physics in our universities restricted their work to undergraduate teaching and in only a few laboratories was scientific investigation actually under way. Industrial research laboratories were not even mentioned as a possibility, and general public interest in physics simply did not exist.

Edward Leamington Nichols was one of the small group of physicists who "carried on" in spite of all discouragement and whose enthusiasm and persistent effort finally brought about the increased activity and interest in physics which, beginning about 1890, is so much in evidence today. Nichols was a pioneer in several branches of physics and the results of his experimental work were not only important but often of such a character as to arouse wide interest. As a teacher he had the power of arousing the lasting enthusiasm of his students, many of whom themselves became teachers and spread his influence still further. Prominent and active in many scientific organizations, he kept continually before the public the cultural and practical importance of physics and the dignity of scientific research in general. By founding the *Physical Review* he stimulated research by providing a place for its publication. In all these ways he helped. But back of all these activities, and more important than any of them, were his firm belief in the value of scientific research, his enthusiasm for experimentation, his sympathetic and helpful interest in the work of others, and a host of personal characteristics which won the respect and affection of his students and associates.

Edward Leamington Nichols was born on September 14, 1854 in Leamington, England, where his parents were making a prolonged stay. The Nichols family had come to this country from

England early in the eighteenth century and his ancestors on his mother's side at about the same time. Nichols's grandfather, Noah N., was a Baptist minister in Boston. His father, Edward W. Nichols, for a time a teacher of music and a student of law, was later a successful landscape painter, member of the Academy of Design. Of his paintings, now chiefly in private collections, about twenty are listed in the exhibition catalogues of the Academy. The mother of Edward L. Nichols, Maria Wilkinson, from Hartford, Conn., had spent some time not long before her marriage as a volunteer teacher in a missionary school in Smyrna.

For a number of years Nichols's parents lived abroad, in Italy, France and England, always in some locality that offered opportunities to an artist for sketching or study. It was during a two years' stay in Leamington, England, that their only child, Edward Leamington Nichols, was born.

Even so brief an outline of his ancestry suggests the basis for many of the traits of character that were so important in Nichols's career—the wide range of his interests, for example, which prevented him from becoming a narrow specialist and which added so much to his power of sympathetic helpfulness as a teacher. In the firmness of his convictions, also, he showed the effect of his New England ancestry, but he was made tolerant by his wide interests and his taste for music and art. Even his enthusiasm for foreign travel seems to have a basis in heredity.

Returning to America when Nichols was only a few years of age the family lived in New Jersey, in Peekskill, and frequently during the summer in the New England mountains. Nichols went to school first in Orange, N. J., and prepared for college at Peekskill Military Academy. He entered Cornell University in 1871 and was graduated in 1875. There seems to have been nothing in his early life to suggest a special interest in science. At Cornell, however, he became much interested in chemistry and toward the end of his course still more strongly interested in physics. There can be little doubt that it was the influence of Professor William A. Anthony that led to his choice of physics as a career. Although not himself an investigator in any modern sense, Anthony was thoroughly abreast of his time—in many

ways far ahead. His lectures were illustrated by experiments and Cornell was one of the few colleges which were at that time beginning to offer laboratory instruction in physics. It was while Nichols was at Cornell also that Anthony built a gramme dynamo, one of the first in this country, and established an electric lighting system for the campus. There was no routine, nothing standardized, about Nichols's first contact with physics; it was development work in a new field, for teacher and student alike. It is easy to see how this pioneer work, in close contact with an able teacher, must have appealed to his love of adventure and aroused his interest and enthusiasm.

After graduation at Cornell, Nichols spent four years in Germany, studying first in Leipzig with Gustav Wiedemann, then for two years in Berlin with Helmholtz and Kirchoff, and finally at Göttingen, where he received the degree of Ph.D. in 1879. His stay in Germany served to broaden his interests still further and gave him a firm belief in the importance of research and in the liberal spirit then so characteristic of the German universities.

Upon his return to America he found this country still suffering from the effects of the panic of 1873 and had difficulty in finding a position. At the suggestion of Andrew D. White, then president of Cornell, he applied for a fellowship at Johns Hopkins and upon receiving appointment devoted the year 1879-80 to the repetition of Rowland's experiment on the magnetic effect of a moving charge. He had had some experience with Rowland's original apparatus while still in Berlin. Nichols's work led to a better understanding of the reasons for the contradictory results that had been obtained and helped eliminate some of the sources of error in this important experiment. But it was not until the work of Crémieu and Pender that the sources of confusion were finally removed.

The following year Nichols was one of Edison's assistants in the famous Menlo Park Laboratory. His special work there was the development of photometric methods for use with the incandescent lamp, then just coming to its practical form; but his work broadened so as to touch on most of the projects then under way in the laboratory. His experience with Edison un-

doubtedly increased his interest in experimental work in new fields.

Nichols's first experience in teaching was at Central University, Kentucky, where he held the chair of physics and chemistry from 1881 to 1883. He was called to the University of Kansas in 1883 and remained there as Professor of Physics and Astronomy until he returned to Cornell as head of the department of physics in 1887. He became Professor Emeritus in 1919.

In 1881, at the beginning of his teaching career, he married Ida Preston of South Dover, N. Y. who had been a fellow student at Cornell during the years 1872-5. Their two children are Elizabeth (Mrs. Montgomery H. Throop), now living in Shanghai, and Robert Preston Nichols of Hollywood, Florida.

Nichols's interest and activity in research, and especially in pioneering work in new fields, was early in evidence. Five papers were published on work done while still in Germany. It is interesting to note that among them was a paper on the color of the sky, a subject to which he returned many years later when, with specially constructed portable apparatus, he made spectrophotometric studies of skylight during a trip around the world. He published four papers while at Johns Hopkins, one a joint paper with Rowland. Even under the unfavorable conditions which then existed at Central University he was able to obtain material for four papers. One of them gave the results of a difficult and dangerous investigation of the undercooling of vapors. Another, far ahead of the times, was on the influence of the electric light on plants.

It was while at the University of Kansas that Nichols's special interest in problems connected with light began to be evident, and from this time on an increasingly large proportion of his papers fall into this field. Undoubtedly his interest in these lines of work had been greatly stimulated by his contact with Edison during the early development period of the incandescent lamp. However, both at Kansas and later at Cornell, his interest in electrical problems remained active. With W. S. Franklin he published several papers on the chemical behavior of iron in a magnetic field, a subject at that time attracting considerable attention. Several other investigations, also published in collaboration with Franklin, dealt with fundamental questions

raised by the Maxwell Theory of Electricity. One of the most important of these was an attempt, attended with great experimental difficulties, to detect evidence of motion in the ether surrounding a moving body. To illustrate the variety of Nichols's interest and the pioneering character of his work attention should be called to a short article which he published during this same period on the "Regulation of Dynamos by Means of a Third Brush"—a method of regulation which in recent years has been extensively used with automobile generators.

The numerous papers on color, physiological optics, and illumination published during the first twenty years of Nichols's scientific activity exerted a great influence upon the development of these fields in America. In recognition of his pioneer work in these fields he was elected an honorary member of the American Optical Society and of the Illuminating Engineering Society. In the case of the latter society, Nichols and Edison were for many years the only recipients of this honor.

The subject of luminescence first attracted Nichols's interest in 1903. From that time on his research work was devoted almost entirely to problems in this general field. In the beginning the writer of this memoir was associated with him in this work, and seventeen papers were published by Nichols and Merritt under the general title "Studies in Luminescence", the last one appearing in 1917. Pressure of other duties, and finally the war, made it impossible for the writer to continue this joint work, and in the continuation of his research in this field Nichols either carried on his experimental work alone or in a collaboration with H. L. Howes, D. T. Wilber, Frances G. Wick, Mabel K. Slattery, or L. J. Boardman.

For many years the work was supported by grants from the Carnegie Institution of Washington, Nichols being a Research Associate of the Institution from 1908 to 1936. The results of his individual work and that of his associates and students were collected and published in book form as Carnegie Publications: *Studies in Luminescence*. Nichols and Merritt, 1912; *Fluorescence of the Uranyl Salts*. Nichols, Howes, Merritt, Wilber and Wick, 1919; *Cathodo-Luminescence*. Nichols, Howes and Wilber, 1928.

All of Nichols's research work had to do either with something that was altogether new or with some new aspect of phenomena already known. He was essentially a pioneer, both in his interest and in his mode of approach. Much of his work called for manipulative skill of a high order; all of it called for ingenuity in meeting new problems. But when an investigation reached a point where high precision was called for he was ready to go on to something beyond. Perhaps his summers spent in the Colorado mountains during his early years had had an influence on his scientific tastes. He was not interested in building roads or in making detailed topographical maps—valuable as he recognized this work to be. What interested him more was climbing by the best route available to the next hill top to get a glimpse of what was beyond.

Next to the results of his own research work and the stimulus of his enthusiasm Nichols's most outstanding contribution to American physics was the establishment of the *Physical Review* in 1893. Previous to that time there was in this country no journal devoted exclusively—or even primarily—to physics and the need of more adequate provision for publication had been keenly felt. With the financial support of Cornell University, Nichols established such a journal six years before the American Physical Society was organized and when the number of active physicists in this country was far too small to make any cooperative plan of publication practicable. He remained editor-in-chief for twenty years and during this period the growth of the *Review*, both in circulation and in size, was rapid and continuous. Even before the organization of the Physical Society the material to be published increased more rapidly than the income and it was not until 1910 that the journal became selfsupporting. In 1913, after a balanced budget had been maintained for two years and when the American Physical Society, with six hundred members, had become strong enough to ensure its continued support, Nichols retired from his duties as editor and the *Review* was transferred to the Society.

Nichols's enthusiasm for his subject and his friendly and sympathetic interest in the problems and difficulties of his students made him a most inspiring teacher. Not long after he became head of the department at Cornell, in 1887, it was my

good fortune to spend a year as a graduate student under his guidance and I can well understand the reason for the respect and affection which all his students since have felt for him. Graduate students began to come to him in increasing numbers as soon as he returned to Cornell, and in only a few years his laboratory there became an important center of advanced study and research. The weekly meetings of the Journal Club and Seminary for advanced students were held in the Nichols home on the university campus and with the understanding help of Mrs. Nichols were made occasions which no member would willingly miss. For many years also Mr. and Mrs. Nichols were at "home" to give a cordial welcome to all advanced students and staff members every Thursday evening. In this way, and because of Nichols's frank and cordial attitude in all his contacts with his students, the workers in his laboratory came to form almost a family group. The result was not only the creation of a pleasant social atmosphere in the department but also a broadening of the scientific interests of the members of the group and the creation of new opportunities for Nichols's influence to make itself felt. Undoubtedly one of his most important contributions to American physics was the indirect influence that he exerted through the students who had received their inspiration and their scientific ideals from him and who later entered the field of college teaching or industrial physics. At the time of his retirement the heads of the departments of physics in thirty-five colleges, fifteen of them state universities, were men who had received their physics training from him. Add to this list the large number of his students who held important posts in government and industrial laboratories or who were college teachers but not department heads, and we get some idea of how great his indirect influence was.

Although Nichols enjoyed teaching, especially when it meant personal contact with advanced students, he greatly disliked the administrative side of his university duties, and this in spite of the fact that he was highly successful in this work. His success came, I think, from his thoroughly democratic attitude toward the problems of administration. Administrative duties and responsibilities were shared with his colleagues in the department and every encouragement was given to individual initiative.

Although he would have preferred to devote his whole time to research he took his duties as a member of the faculty seriously and his wise and altogether unprejudiced approach to the general educational problems of the university made him a most influential and valuable faculty member. When a plan for faculty representation on the University Board of Trustees was adopted, Nichols was one of the first to be elected by the faculty as one of its representatives. His distaste for administrative work and his dislike—which almost amounted to resentment—for over organization in scientific work are evident in the following quotation from his address as retiring president of the American Association for the Advancement of Science in 1908:

“When in any of our institutions a man distinguishes himself by productive work he is frequently made a dean, director or even president, and is thus retired from what might have been a great career as an investigator. Thereafter he is compelled to devote himself to administrative duties, which some one not equipped for the important task of adding to the world’s stock of knowledge might just as well perform. It is as though the authorities were to say: ‘X has written an admirable book; we must appoint him bookkeeper—or Y is developing a decided genius for landscape; we will increase his salary and ask him to devote all his time to painting the woodwork of the university buildings.’ Nor does the mischief stop with the sacrifice of a few bright spirits. It extends to the bottom. The head of each department is a petty dean, cumbered with administrative detail. He is expected to hold every one under him to account, not for scholarly productiveness, but for the things which chiefly hinder it.”

“In this exaltation of administrative ability over creative gifts which are much rarer and more precious, our institutions share the weakness which pervades our industrial establishments. In both we see the same striving for a certain sort of efficiency and economy of operation and for the attainment of a completely standardized product. This tends in both cases to the elimination of individuality and to sterility. In the University it retards instead of developing research. In industry it discourages originality.”

At the dinner given in Nichols’s honor at the time of his retirement in 1919 it was a matter for humorous comment that five of the seven speakers were either college presidents or

deans whose appointment had resulted from their success in other than administrative work; and of the three deans Nichols himself was one! But Nichols's acceptance of the position of Dean of the College of Arts and Sciences may almost be said to have been forced upon him. Upon the initiative of President Schurman the office had been made elective and with a two years' term. It was expected that a permanent staff would be organized for the handling of administrative detail and that the dean would be concerned only with broad questions of policy. With this understanding, and because the democratic character of the original plan made a strong appeal to him, Nichols accepted election as the first dean under the new plan.

Even in the early years of his scientific career Nichols fully appreciated the practical value of scientific research. The fact that research furnishes the foundation on which the applications of science in industry must build is now generally appreciated and it is rarely necessary to stress the point. But this was not true sixty years ago. Probably only the members of Nichols's own generation are in a position fully to realize how great the change has been. The attitude of the general public toward those who devoted themselves to scientific research was then merely one of kindly tolerance. What the scientist did was recognized as harmless and sometimes interesting. But his work was regarded as having no relation to the affairs of every day life. In his teaching, in his public addresses,—in every way possible—Nichols exerted himself to correct this misapprehension and to point out the practical value of pure science. He went further than this and repeatedly called attention to the fact that those nations and communities in which there is activity in pure science research are usually the ones in which progress in the application of scientific knowledge is most rapid. To quote again from his presidential address before the American Association:

“A country that has many investigators will have many inventors also. A scientific atmosphere dense enough to permeate the masses brings proper suggestions to many practically inclined minds. Where science is there will its by-product, technology, be also. Communities having the most thorough fundamental knowledge of pure science will show the greatest

output of really practical inventions. Peoples who get their knowledge at second-hand must be content to follow."

\*       \*       \*       \*       \*       \*       \*

"Nearly all really important technical advances have their origin in communities where the great fundamental sciences are most extensively and successfully cultivated."

Many interesting illustrations are then given to support this statement.

A firm believer in the value of scientific research to humanity, Nichols also greatly enjoyed his experimental work for its own sake. One might almost say that his belief in the value of science was as much the justification as the cause of his scientific activity. In my long association with him I was again and again impressed by the vigorous and almost joyous way in which he met and overcame experimental difficulties and by his enthusiastic welcome of new and unexpected results. His attitude was that of a young athlete engaged in a game which called for all his energy and skill, and which for that very reason he thoroughly enjoyed. No one could be associated with him long either as student or colleague without acquiring in some degree this same attitude toward scientific work.

After having served for thirty-two years as head of the department of physics at Cornell, Nichols retired from active teaching in 1919, at the age of 65. His scientific activity, however, was by no means at an end. Between 1919 and 1936 he published thirty-seven papers, all but five of which dealt with the results of his own experimental work. During the greater part of this period also he was active in guiding the work of his assistants and in helping by his suggestions and advice former students, now connected with other colleges, who returned each summer to use the special equipment and the large amount of experimental material which he had accumulated for work in the luminescence field. His presence in the laboratory was a continuing stimulus to the graduate students and the members of the staff.

It was Nichols's habit to devote himself to his experimental work as continuously as his other duties would permit until the need and opportunity for rest came—and then to drop his

scientific work for a time completely. He greatly enjoyed foreign travel and usually spent his sabbatic leave in Europe. On several occasions, however, more extended trips were made, and at one time or another he visited each of the six continents. During the later years, after failing eyesight seriously interfered with his experimental work, and when the health of Mrs. Nichols made a milder climate desirable, he spent much of his time in Florida. It was in West Palm Beach, Florida, at the home of his son, Robert, that he died, November 10, 1937, in his 84th year.

Throughout his life Nichols was a member of the Episcopal Church and for many years he was a member of the vestry of St. John's Church in Ithaca. The difficulties that some have felt in reconciling the results of scientific discovery with their religious belief did not exist for him. He saw no "conflict" between religion and science. It is not easy to summarize his views on such questions, for although he sometimes gave informal talks on the relation of science and religion he published only one short article on this subject. The following quotations from this article are, however, helpful:

"Science \* \* \* strives to give an account, intelligible and systematic, of the world in which we live—and, so far as physical science is concerned—solely of the material world."

"Science has certain important by-products: Engineering is such a by-product, \* \* \* Citizenship is another, and I propose to show that science tends to produce a religious citizenship."

"The normal man of science of to-day may, rarely, be an agnostic; never an atheist. He is more likely to be, and generally is, a Christian of profound religious feeling."

"Thoughtful contemplation of the material universe \* \* \* leads inevitably to belief in an intelligent creator, \* \* \* without the aid of theology or of any revelation aside from that afforded by the material universe and our relations to it we reach the idea of a personal God \* \* \*."

"Science demands of its followers as the conditions of the highest success certain characteristics that are no less essential to the religious citizen \* \* \*. Such are a passion for knowledge, the love of truth, honesty, patience, singleness of mind, simplicity of character, humility, reverence, imagination. This list of great attributes, to be sure, cannot be ascribed to all men of science—not all citizens are good citizens!—but

search the lives of the truly great in science and you will find these characteristics represented in notable degree."

I am sure that Nichols felt that dogmatism is no more justified in the scientist than in the theologian: that we have penetrated so short a distance into the unknown that while the scientist may well speak of some seeming violation of natural law as extremely improbable, he is never justified in using the word impossible. I have several times heard him comment on the fact that many of the scientific facts that are almost commonplace today would have seemed, only a few centuries ago, as impossible as do any of the miracles of the Bible. Of greater importance, however, in the case of his own religious beliefs was, I think, his feeling that the subject matter of science is altogether different from that of religion; and that science, which deals only with material things, can therefore throw little light on the questions that are dealt with by religion. His attitude toward the beliefs of others was one of sincere and very real tolerance.

In recognition of his scientific work Nichols was awarded the Elliott Cresson Medal of the Franklin Institute, the Ives Medal of the Optical Society, and the Rumford Medal of the American Academy (1928) and was made an honorary member of the Illuminating Engineering Society and of the American Optical Society. Honorary degrees were conferred upon him by the University of Pennsylvania (1908) and Dartmouth (1910). Among the scientific societies of which he was a member, and in most cases a very active member, were the American Association for the Advancement of Science, the Illuminating Engineering Society, the American Optical Society, the Institute of Electrical Engineers, the American Philosophical Society, the American Academy of Arts and Sciences, and the National Academy of Sciences. He served as president of the Kansas Academy in 1885, of the National Society of Sigma Xi in 1908, the American Association for the Advancement of Science in 1907, the American Physical Society (1907-1909).

# KEY TO ABBREVIATIONS

- Amer. Inst. Elec. Eng. Trans.—American Institute of Electrical Engineers Transactions.  
 Amer. Jl. Sci.—American Journal of Science.  
 Amer. Phil. Soc. Proc.—American Philosophical Society Proceedings.  
 Bull. Nat. Res. Council—Bulletin, National Research Council.  
 Elec. Soc. & Soc. Mech. Eng., Cornell Univ.—Electric Society and Society of Mechanical Engineers, Cornell University.  
 Illum. Eng. Soc. Trans.—Illuminating Engineering Society Transactions.  
 Int. Elec. Cong. St. Louis—International Electrical Congress, St. Louis.  
 Jour. Franklin Inst.—Journal of the Franklin Institute.  
 Jour. Opt. Soc. Amer.—Journal, Optical Society of America.  
 Kans. Acad. Sci. Trans.—Kansas Academy of Sciences Transactions.  
 Nat. Acad. Sci. Biog. Mem.—National Academy of Sciences Biographical Memoirs.  
 Nat. Acad. Sci. Proc.—National Academy of Sciences Proceedings.  
 Phil. Mag.—Philosophical Magazine.  
 Phys. Rev.—Physical Review.  
 Physikalische Zs.—Physikalische Zeitschrift.  
 Póp. Sci. Mo.—Popular Science Monthly.  
 Proc. Amer. Assn. Adv. Sci.—Proceedings American Association for the Advancement of Science.  
 Sci. Abs.—Science Abstracts.  
 Sci. Mo.—Scientific Monthly.  
 Sibley Jour. Eng.—Sibley Journal of Engineering.

# BIBLIOGRAPHY

1878

- Ueber die Volumenvermehrung der Fluessigkeiten durch Absorption von Gasen. (With J. J. Mackenzie.) *Annalen d. Physik u. Chemie*, n.f., Bd. III, pp. 134-142.

1879

- Ueber das von gluehenden Platin ausgestrahlte Licht. Goettingen: Huth. 58 pp. (Thesis.)  
 On the character and intensity of the rays emitted by glowing platinum. *Amer. Jl. Sci.*, vol. 18, ser. 3, pp. 446-468, Dec.  
 A new explanation of the colour of the sky. *Phil. Mag.*, vol. 8, ser. 5, pp. 425-433, Dec.

1880

- Upon an optical method for the measurement of high temperatures. *Amer. Jl. Sci.*, vol. 19, pp. 42-49, Jan.  
 Salt solutions and the absorption of gases. *Proc. Amer. Assn. Adv. Sci.*, vol. 29, pp. 324-327. Aug.

1881

- On the coefficient of expansion of gas-solutions. (With A. W. Wheeler.)  
Phil. Mag., Feb., pp. 113-120.  
Electric absorption of crystals. (With H. A. Rowland.) Phil. Mag.,  
June, pp. 414-419.  
Note on the electrical resistance and the coefficient of expansion of incan-  
descent platinum. Amer. Jl. Sci., vol. 22, pp. 363-368, Nov.

1882

- Earthworms and their influence upon the soil. Kentucky State Agricul-  
tural Report, pp. 44-54.  
Influence of the electric light upon growing plants. Kentucky State Agri-  
cultural Report, pp. 54-61.

1884

- Experiments upon super-heated liquids and upon the supersaturation of  
vapors. Kans. Acad. Sci. Trans., vol. 9, pp. 91-95.  
Statistics on color-blindness in the University of Kansas. Kans. Acad.  
Sci. Trans., vol. 9, pp. 95-96.  
A preliminary note upon the action of acids upon iron in the magnetic  
field. Proc. Amer. Assn. Adv. Sci., vol. 33.  
On the duration of color impressions upon the retina. Amer. Jl. Sci., vol.  
28, ser. 3, pp. 243-252, Oct.  
A spectro-photometric study of pigments. Amer. Jl. Sci., vol. 28, ser. 3,  
pp. 342-348, Nov.

1885

- A spectro-photometric analysis of the color of the sky. Proc. Amer.  
Assn. Adv. Sci., vol. 34, pp. 78-79.  
On the chemical behavior of iron in the magnetic field. Proc. Amer.  
Assn. Adv. Sci., vol. 34, pp. 79-80.  
On the sensitiveness of the eye to colors of a low degree of saturation.  
Amer. Jl. Sci., vol. 30, pp. 37-41, July.

1886

- On the chemical behavior of iron in the magnetic field. Amer. Jl. Sci.,  
vol. 31, ser. 3, pp. 272-83, Apr.

1887

- On the delicacy of the sense of smell. (With E. H. S. Bailey.) Nature,  
vol. 37, p. 557.  
On black and white. Kans. Acad. Sci. Trans., vol. 10, pp. 37-44.  
The sky. Kans. Acad. Sci. Trans., vol. 10, pp. 111-121.  
On the destruction of the passivity of iron in nitric acid by magnetization.  
(With W. S. Franklin.) Amer. Jl. Sci., vol. 34, ser. 3, pp. 419-427,  
Dec. (Also in the Trans., Kans. Acad. Sci.)

1888

- The electromotive force of magnetization. (With W. S. Franklin.)  
 Amer. Jl. Sci., vol. 35, pp. 290-297, Apr. .
- The Holtz machine as a motor; historical sketch. *Electrical World*, vol. 12, p. 53, Aug. 4.
- On compensated resistance standards. *Amer. Inst. Elec. Eng. Trans.*, vol. 5, pp. 296-304.
- On the delicacy of the sense of taste. (With E. H. S. Bailey.) *Science*, 1888, also in *Nature* 1888. (Presented at N. Y. meeting of the Amer. Assn. Adv. Sci., Aug. 12, 1887.)

1889

- An experiment bearing upon the question of the direction and velocity of the electric current. (With W. S. Franklin.) *Amer. Jl. Sci.*, vol. 37, pp. 103-109, Feb.
- A spectro-photometric comparison of sources of artificial illumination. (With W. S. Franklin.) *Amer. Jl. Sci.*, vol. 38, ser. 3, pp. 100-114, Aug.
- A preliminary report on the influence of temperature upon the color of pigments. (With B. W. Snow.) *Abstract. Proc. Amer. Assn. Adv. Sci.*, vol. 38, p. 133.
- The efficiency of methods of artificial illumination. *Amer. Inst. Elec. Eng. Trans.*, vol. 6, pp. 158-9, discussion, pp. 190-202.
- The personal error in photometry. *Amer. Inst. Elec. Eng. Trans.*, vol. 6, pp. 335-341.
- The regulation of dynamos by means of a third brush. *Electrician*, London, vol. 22, p. 440.

1890

- The electrical resistance of the alloys of ferromanganese and copper. (From determinations made by Mr. B. H. Blood.) *Amer. Jl. Sci.*, vol. 39, pp. 471-77, June.
- Radiation at red heat. Preliminary note on the radiation from zinc oxide. (With B. W. Snow.) *Proc. Amer. Assn. Adv. Sci.*, vol. 39, p. 92.
- Note on a new photometer. *Amer. Inst. Elec. Eng. Trans.*, vol. 7, pp. 257-262, discussion pp. 262-4.
- The artificial light of the future. Paper read before the Electric Club, Nov. 20, 1890. 31 pp.
- Two problems in the physics of photography. *Chautauquan*, vol. 10, p. 673.
- The production of artificial cold. *Chautauquan*, vol. 11, p. 42.
- Color blindness. *Chautauquan*, vol. 11, May.
- How electricity is measured. *Chautauquan*, vol. 11, June.

1891

- On the alternating electric arc between a ball and a point. *Amer. Jl. Sci.*, vol. 41, pp. 1-11, January.

On the influence of temperature on the colour of pigments. (With B. W. Snow.) *Phil. Mag.*, vol. 32, ser. 5, pp. 401-422, November.

A photographic study of the electric arc. *Amer. Inst. Elec. Eng. Trans.*, vol. 8, pp. 214-221, discussion pp. 222-6.

1892

On the character of the light emitted by incandescent zinc oxide. *Phil. Mag.*, January, pp. 19-28.

Note on the selective absorption of light by optical glass and calc-spar. (With B. W. Snow.) *Phil. Mag.*, April, pp. 379-382.

The age-coating in incandescent lamps. *Amer. Jl. Sci.*, vol. 44, pp. 277-285, October.

1893

Some applications of electric heating in physical laboratory practice. *Phys. Rev.*, vol. 1, pp. 144-151, Sept.-Oct.

John Tyndall, a biographical notice. *Phys. Rev.*, vol. 1, p. 302.

Phenomena of the Time-Infinitesimal. *Proc. Amer. Assn. Adv. Sci.*, vol. 42, pp. 57-71. (Address of the Vice-president of Section B.)

An apparatus for the generation of oxygen and hydrogen by electrolysis. (With G. S. Moler.) *Abstract. Proc. Amer. Assn. Adv. Sci.*, vol. 42, p. 79.

1894

An apparatus for the generation of oxygen and hydrogen by electrolysis. (With G. S. Moler.) *Phys. Rev.*, vol. 1, pp. 373-380, March-April.

On the condition of the ether surrounding a moving body. (With Wm. S. Franklin.) *Phys. Rev.*, vol. 1, pp. 426-441, May-June.

August Kundt, a biographical notice. *Phys. Rev.*, vol. 2, p. 68.

A new form of spectro-photometer. *Phys. Rev.*, vol. 2, pp. 138-141, Sept.-Oct.

Studies of the lime light. (With Mary L. Crehore.) *Phys. Rev.*, vol. 2, pp. 161-169, Nov.-Dec.

Hermann von Helmholtz. *Phys. Rev.*, vol. 2, pp. 222-227, Nov.-Dec.

1895

The distribution of energy in the spectrum of the glow lamp. *Phys. Rev.*, vol. 2, pp. 260-276.

The influence of temperature on the transparency of solutions. (With Mary C. Spencer.) *Phys. Rev.*, vol. 2, p. 344.

The new physics laboratory at Lille. *Phys. Rev.*, vol. 3, pp. 232-233, Nov.-Dec.

1896

Photography of concealed objects; the Roentgen effect. Read before the Albany Institute, Mar. 3. 10 pp.

Standards of light. Preliminary report of the subcommittee of the Institute. (With C. H. Sharp and C. P. Matthews.) *Amer. Inst. Elec. Eng. Trans.*, vol. 13, pp. 133-198, with discussion pp. 198-205.

1897

The influence of a static charge of electricity upon the surface tension of water. (With John A. Clark.) *Phys. Rev.*, vol. 4, pp. 375-387, March-April.

1898

William A. Rogers. *Phys. Rev.*, vol. 6, pp. 315-319, May-June.

The photography of manometric flames. (With E. Merritt.) *Phys. Rev.*, vol. 7, pp. 93-101, August.

The use of the lantern in science teaching. *School Journal*, vol. 57, p. 684.

1899

On the density of ice. I. A review of the existing data. *Phys. Rev.*, vol. 8, pp. 21-37, January.

Note on the coefficient of expansion of ice. *Phys. Rev.*, vol. 8, pp. 184-186, March.

Gustav Wiedemann. *Phys. Rev.*, vol. 9, pp. 57-58, July.

Wilhelm Gottlieb Hankel. *Phys. Rev.*, vol. 9, p. 58, July.

Robert Wilhelm Bunsen. *Phys. Rev.*, vol. 9, pp. 310-312, Nov.-Dec.

1900

On the temperature of the acetylene flame. *Phys. Rev.*, vol. 10, pp. 234-252, April.

Ueber die Temperatur der Acetylenflamme. *Physikalische Zs.*, Jarg. 2, #3, s. 34-36.

Preliminary note on the efficiency of the acetylene flame. *Phys. Rev.*, vol. 11, pp. 215-229, October.

Vorlaefige Notiz ueber den Wirkungsgrad der Acetylenflamme. *Physikalische Zs.*, Jarg. 2, s. 221-223.

The acetylene flame. *Jour. Franklin Inst.*, vol. 150, pp. 356-387, November.

The electric and magnetic properties of matter at low temperature, a résumé. *Elec. Soc. & Soc. Mech. Eng.*, Cornell Univ., Ithaca 1900-01, vol. 8, pp. 87-92.

On a new method of calibrating thermo-electric elements for use in the measurement of high temperatures. *Archives Neerlandaises des Sciences Exactes et Naturelles*, vol. 5, ser. 2, pp. 339-347, reviewed in *Sci. Abs.*, vol. 4, p. 1289.

1901

Henry Augustus Rowland, physicist. *Phys. Rev.*, vol. 13, pp. 60-64, July.

The visible radiation from carbon. I. *Phys. Rev.*, vol. 13, pp. 65-80, August.

The visible radiation from carbon. II. *Phys. Rev.*, vol. 13, pp. 129-144, September.

A further note on the visible radiation from carbon. (With E. Blaker.) *Phys. Rev.*, vol. 13, pp. 378-380, December.

Research work for physics teachers. *Science*, n. s., vol. 13, pp. 202-208, February 8.

1902

- Rudolph Koenig. *Phys. Rev.*, vol. 14, pp. 62-63, January.  
On some optical properties of asphalt. *Phys. Rev.*, vol. 14, pp. 204-213, April.

1903

- Ogden Nicholas Rood. *Phys. Rev.*, vol. 16, pp. 311-313, May.  
On methods of measuring radiant efficiencies. (With W. W. Coblentz.) *Phys. Rev.*, vol. 17, pp. 267-276, October.

1904

- The phosphorescence of organic substances at low temperatures, preliminary note. (With E. Merritt.) *Phys. Rev.*, vol. 18, pp. 120-122, February.  
The spectro-photometric study of fluorescences. (With E. Merritt.) *Phys. Rev.*, vol. 18, pp. 122-123, February.  
Studies in luminescence. I. The phosphorescence and fluorescence of organic substances at low temperatures. (With E. Merritt.) *Phys. Rev.*, vol. 18, pp. 355-365, May.  
The effect of light upon the absorption and the electrical conductivity of fluorescent solutions. (With E. Merritt.) *Phys. Rev.*, vol. 18, pp. 447-449, June.  
Studies in luminescence. II. A spectro-photometric study of fluorescent solutions belonging to Lommel's first class. (With E. Merritt.) *Phys. Rev.*, vol. 18, pp. 403-418, June.  
Studies in luminescence. III. On fluorescence spectra. (With E. Merritt.) *Phys. Rev.*, vol. 19, pp. 18-36, July.  
The fundamental concepts of physical science. *Pop. Sci. Mo.*, vol. 66, pp. 56-65, November.  
Studies in luminescence. IV. The influence of light upon the absorption and electrical conductivity of fluorescent solutions. (With E. Merritt.) *Phys. Rev.*, vol. 19, pp. 396-421, December.  
Standards of light. *Int. Elec. Cong. St. Louis*, vol. 2, pp. 811-831.  
The influence of low temperature upon certain color indicators. (With E. Merritt.) In: *Boltzmann-Festschrift, Leipzig: Barth*, 1904, pp. 890-898.

1905

- The luminescence of Sidot blende. (With E. Merritt.) *Phys. Rev.*, vol. 20, pp. 120-122, February.  
On the distribution of energy in the visible spectrum. *Phys. Rev.*, vol. 21, pp. 147-165, September.  
Studies of luminescence. V. The luminescence of Sidot blende. (With E. Merritt.) *Phys. Rev.*, vol. 21, pp. 247-259, October.  
The decay of phosphorescence. (With E. Merritt.) *Phys. Rev.*, vol. 21, pp. 410-411, December.  
Die neuere Forschung ueber die Physik der Fluoreszenz, uebersetzt von Max Ikle. *Jahrb. d. Radioaktivitaet & Elektronik Bd. 2, #2*, pp. 1-38.

Physics teaching in the secondary schools, past, present, and future. Eastern Assn. Physics Teachers, 43d meeting, Dec. 9, 1905, Report, pp. 38-45.

1906

Franklin as a man of science. Independent, vol. 60, pp. 79-84, January 11.  
Franklin's researches in electricity. In: Amer. Phil. Soc.: The record of the celebration of the two hundredth anniversary of the birth of Benjamin Franklin, vol. 1, pp. 103-121.

Studies in luminescence. VI. The decay of phosphorescence in Sidot blende. (With E. Merritt.) Phys. Rev., vol. 22, pp. 279-293, May.

Studies in luminescence. VII. Further experiments on the decay of phosphorescence in Sidot blende and certain other substances. (With E. Merritt.) Phys. Rev., vol. 23, pp. 37-54, July.

Fluorescence and phosphorescence. Jour. Franklin Inst., vol. 162, pp. 219-238, Sept.

The mechanical equivalent of light. Repr. from Electr. Mag., London, and from Illuminating Engineer, N. Y.

1907

The scientific work of Dewitt Bristol Brace. Phys. Rev., vol. 24, pp. 515-521, June.

Studies in luminescence. VIII. The influence of the red and infra-red rays upon the photoluminescence of Sidot blende. (With E. Merritt.) Phys. Rev., vol. 25, pp. 362-381, November.

1908

Professor Anthony at Cornell. Sibley Jour. Eng., vol. 22, pp. 229-232, Apr.

Daylight and artificial light. Illum. Eng. Soc. Trans., vol. 3, pp. 301-332.

Theories of the color of the sky. Phys. Rev., vol. 26, pp. 497-511, June.

In memoriam: the Rt. Hon. William Thomson, Lord Kelvin. Amer. Inst. Elec. Eng., N. Y. 1908. 33 pp.

Lord Kelvin as a scientist. In: Kelvin, Wm. Thomson, 1st baron: *In memoriam*. 1908.

Studies in luminescence. X. The phenomena of phosphorescence considered from the standpoint of the dissociation theory. (With E. Merritt.) Phys. Rev., vol. 27, pp. 367-399, Nov.

1909

Science and the practical problems of the future. Science, n.s., vol. 29, pp. 1-10, Jan. 1.

A study of overcast skies. Phys. Rev., vol. 28, pp. 122-131, Feb.

Studies in luminescence. IX. A spectrophotometric study of certain cases of kathodo-luminescence. (With E. Merritt.) Phys. Rev., vol. 28, pp. 349-360, May.

Biographical memoir of Ogden Nicholas Rood, 1831-1902. Nat. Acad. Sci.: Biog. Mem., vol. 6, pp. 449-472, Aug.

- Studies in luminescence. XI. The distribution of energy in fluorescence spectra. (With E. Merritt.) *Phys. Rev.*, vol. 30, pp. 328-346, March.
- The effect of temperature on phosphorescence and fluorescence. *Amer. Phil. Soc. Proc.*, vol. 49, pp. 267-280, Aug.-Sept.
- Studies in luminescence. XII. The absorption of alcoholic solutions of eosin and resorufin. (With E. Merritt.) *Phys. Rev.*, vol. 31, pp. 376-380, Oct.
- Studies in luminescence. XIII. The specific exciting power of the different wave-lengths of the visible spectrum in the case of the fluorescence of eosin and resorufin. (With E. Merritt.) *Phys. Rev.*, vol. 31, pp. 381-387, Oct.
- Studies in luminescence. XIV. Further experiments on fluorescence absorption. (With E. Merritt.) *Phys. Rev.*, vol. 31, pp. 500-512, Nov.
- Some notes on the early history of standards of light. *Illum. Eng. Soc. Trans.*, vol. 5, pp. 842-853, Dec. 8.

1911

- Studies in luminescence. XV. On fluorescence and phosphorescence between  $+20^{\circ}$  and  $-190^{\circ}$ . (With E. Merritt.) *Phys. Rev.*, vol. 32, pp. 38-53, Jan.
- Some recent advances in fluorescence and phosphorescence. *Science*, vol. 33, n.s., pp. 696-700, May 5.
- Studies in luminescence. XVI. The fluorescence and absorption of certain uranyl salts. (With E. Merritt.) *Phys. Rev.*, vol. 33, pp. 354-378, Nov.

1912

- Daylight. *Jour. Franklin Inst.*, Apr., pp. 315-332.
- A method of using the photoelectric cell in photometry. (With E. Merritt.) *Phys. Rev.*, vol. 34, pp. 475-476, June.

1913

- La lumière du jour. *Revue scientifique*, Nov. 15, pp. 615-623.
- Physical science and religious citizenship. *Religious Education*, vol. 8, pp. 424-425, Dec.

1914

- Note on the fluorescence of frozen solutions of the uranyl salts. (With E. Merritt.) *Phys. Rev.*, n.s., vol. 3, pp. 457-463, June.
- The luminescence of kunzite. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 4, pp. 18-27, July.

1915

- Biographical memoir of Henry Morton. *Nat. Acad. Sci. Biog. Mem.*, vol. 8, pp. 143-151, Feb.
- A new fluorescence spectrum of uranyl ammonio-chloride. (With E. Merritt.) *Phys. Rev.*, n.s., vol. 6, pp. 358-376, Nov.
- The polarized fluorescence of ammonium uranyl chloride. (With H. L. Howes.) *Nat. Acad. Sci. Proc.*, vol. 1, pp. 444-450, Aug. 15.

1916

- A synchrono-phosphoroscope. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 7, p. 586, May.
- A new form of phosphoroscope. (With H. L. Howes.) *Science*, n.s., vol. 43, pp. 937-939, June 30.
- Note on the phosphorescence of uranyl salts. *Nat. Acad. Sci. Proc.*, vol. 2, pp. 328-333, June.
- The fluorescence and absorption of certain pleochroic crystals of the uranyl salts. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 8, pp. 364-385, Oct.
- Color-photographs of the phosphorescence of certain metallic sulphides. *Amer. Phil. Soc. Proc.*, vol. 55, pp. 494-502.

1917

- The influence of water of crystallization upon the fluorescence and absorption spectra of uranyl nitrate. (With E. Merritt.) *Phys. Rev.*, n.s., vol. 9, pp. 113-126, Feb.
- New data on the phosphorescence of certain sulphides. *Nat. Acad. Sci. Proc.*, vol. 3, pp. 199-202, March 15.
- On the phosphorescence of the uranyl salts. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 8, pp. 292-304, April.
- Spectral structure of the phosphorescence of certain sulphides. *Amer. Phil. Soc. Proc.*, vol. 56, pp. 258-274.

1918

- Note on a phosphorescent calcite. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 11, p. 327, April.
- On the unpolarized fluorescence and absorption of four double chlorides of uranyl. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 11, pp. 285-310, April.
- The photo-luminescence and kathodo-luminescence of calcite. (With H. L. Howes and D. T. Wilber.) *Phys. Rev.*, n.s., vol. 11, pp. 485-486, June.
- Types of phosphorescence. (With H. L. Howes.) *Nat. Acad. Sci. Proc.*, vol. 4, pp. 305-312, Oct. 15.
- The photo-luminescence and kathodo-luminescence of calcite. (With H. L. Howes and D. T. Wilber.) *Phys. Rev.*, n.s., vol. 12, pp. 351-367, November.

1919

- Fluorescence and absorption of the uranyl acetates. (With H. L. Howes and D. T. Wilber.) *Phys. Rev.*, n.s., vol. 14, pp. 201-221, Sept.
- Fluorescence and absorption of the uranyl sulphates. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 14, pp. 293-305, Oct.

1920

- Luminescence at high temperatures. (With D. T. Wilber.) *Nat. Acad. Sci. Proc.*, vol. 6, pp. 693-695, Dec. 15.

1921

- The luminescence of zinc oxide above the red heat. *Phys. Rev.*, n.s., vol. 17, pp. 429-430, March.  
 Flame excitation of luminescence. (With D. T. Wilber.) *Phys. Rev.*, n.s., vol. 17, pp. 453-468, April.  
 The luminescence of certain oxides sublimed in the electric arc. (With D. T. Wilber.) *Phys. Rev.*, n.s., vol. 17, pp. 707-717, June.  
 Luminescence as a factor in artificial lighting. *Illum. Eng. Soc. Trans.*, vol. 16, pp. 331-345, Oct. 10.  
 Address at the laying of the corner stone of the chemical laboratory of the Cornell University. *Science*, n.s., vol. 54, pp. 651-653, Dec. 30.

1922

- Emission bands of erbium oxide, a confirmation. (With H. L. Howes.) *Science*, n.s., vol. 55, p. 53, Jan. 13.  
 The blue glow. (With H. L. Howes.) *Jour. Opt. Soc. Amer.*, vol. 6, pp. 42-53, January.  
 A convenient method of determining the brightness of luminescence. *Science*, n.s., vol. 35, pp. 157-159, Feb. 10.  
 The luminescence of incandescent solids. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 19, pp. 300-318, April.  
 On Founder's Day. *Sci. Mo.*, vol. 14, pp. 469-474, May.

1923

- Selected topics in the field of luminescence. (With Ernest Merritt and C. D. Child.) *Bull. Nat. Res. Council*, No. 30, vol. 5, part 5, March.  
 The luminescence of incandescent titanium oxide. *Phys. Rev.*, n.s., vol. 21, p. 713, June.  
 Notes on germanium oxide. *Nat. Acad. Sci. Proc.*, vol. 9, pp. 248-251, July 15.  
 The luminescence of titanium oxide. *Phys. Rev.*, n.s., vol. 22, pp. 420-424, Nov.  
 The photoluminescence of flames. I. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 22, pp. 425-431, Nov.

1924

- The photoluminescence of flames. II. (With H. L. Howes.) *Phys. Rev.*, n.s., vol. 23, pp. 472-477, April.  
 The brightness of marine luminescence. *Science*, vol. 60, pp. 592-593, Dec. 26.  
 Report on studies in luminescence. *Carnegie Inst. Year Book* No. 23, pp. 302-305.

1925

- Notes on neodymium oxide. *Nat. Acad. Sci. Proc.*, vol. 11, pp. 47-52, Jan. 15.  
 Visible radiation from columbium oxide. *Phys. Rev.*, n.s., vol. 25, pp. 376-385, March.

1926

Uranium as an activator. (With Mabel K. Slattery.) Jour. Opt. Soc. Amer., vol. 12, pp. 449-466, May.

Note on the rare earths as activators of luminescence. (With H. L. Howes.) Jour. Opt. Soc. Amer., vol. 13, pp. 573-587, Nov.

Links connecting fluorescence and the luminescence of incandescent solids. Jour. Opt. Soc. Amer., vol. 13, pp. 661-677, Dec.

1927

On transformation spectra. (With H. L. Howes and D. T. Wilber.) Jour. Opt. Soc. Amer., vol. 14, pp. 205-222, March.

1928

Biographical memoir of Ernest Fox Nichols, 1869-1924. Nat. Acad. Sci. Biog. Mem., vol. 12, pp. 99-131.

1929

The transformation spectrum of the ruby. (With H. L. Howes.) Nat. Acad. Sci. Proc., vol. 15, pp. 139-146, Feb.

1930

Reversible oxidation in luminescence. Jour. Opt. Soc. Amer., vol. 20, pp. 106-114.

Activators in cando-luminescence. (With L. J. Boardman.) Jour. Opt. Soc. Amer., vol. 20, pp. 115-130.

Ozone in fluorescence. (With Frances G. Wick.) Nat. Acad. Sci. Proc., vol. 16, pp. 32-36.

1932

Thermal behavior of certain solid solutions. (With Mary A. Ewer.) Jour. Opt. Soc. Amer., vol. 22, pp. 456-465.

On temperature crests in cando-luminescence. (With H. L. Howes.) Jour. Opt. Soc. Amer., vol. 22, pp. 170-189.

Relation between certain types of luminescence. (With Frances G. Wick.) Jour. Opt. Soc. Amer., vol. 22, pp. 357-368.

1933

Sidelights on the era of Young and Fresnel. Jour. Opt. Soc. Amer., vol. 23, pp. 1-6.

Hydrogen effect in certain oxides. (With C. L. Stanford.) Jour. Opt. Soc. Amer., vol. 23, p. 120A.

1936

Hydrogen effect in certain oxides. (With C. L. Stanford.) Jour. Opt. Soc. Amer., vol. 26, pp. 91-98.

BOOKS

- The galvanometer. N. Y.: McElroy & Emmet, 1894. 112 pp.
- A laboratory manual of physics and applied electricity. N. Y.: Macmillan, 1895. 2 vol. (Revised editions have been published at various dates.)
- The elements of physics. (With W. S. Franklin.) N. Y.: Macmillan, 1896. 3 vol. (Revised editions have been published at various dates.)
- The outlines of physics, an elementary textbook. N. Y.: Macmillan, 1898.
- Studies in luminescence. (With E. Merritt.) Carnegie Institution of Washington. Publication No. 152. 1912.
- Fluorescence of the uranyl salts. (With H. L. Howes, in collaboration with Ernest Merritt, D. T. Wilber, and Frances G. Wick.) Carnegie Institution of Washington. Publication No. 298. 1919.
- Cathodo-luminescence and the luminescence of incandescent solids. (With H. L. Howes and D. T. Wilber.) Carnegie Institution of Washington. Publication No. 384. 1928.

3614







